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# The American Political Science Review

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## American Political Science Association

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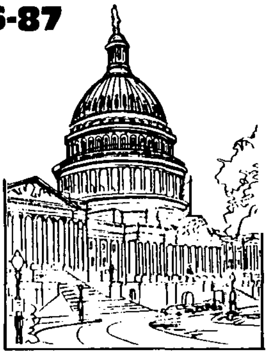

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# Marxism and Secular Faith

RICHARD J. ARNESON
*University of California, San Diego*

*It has been argued by Mancur Olson and others that Karl Marx's theory of revolution is logically defective in that from its premises one cannot draw Marx's conclusion that workers will unite to revolt against capitalism. Workers who might wish for large social changes are confronted with a collective action problem that Marx fails to appreciate—so runs the criticism. The critics are assuming that Marx is reasoning from a Hobbesian premise to the effect that insofar as they are rational, individuals act always to fulfill narrowly self-interested goals. This article denies the assumption. In particular it is urged that to make sense of Marx's optimistic hopes about the likely outcome of successful majoritarian working-class revolution, one must attribute to him a secular faith that most people are disposed to play fair with others. This disposition is relatively weak and only sporadically effective in determining behavior, but in the right revolutionary circumstances, Marx hopes, it might play a considerably greater role in this respect. (A circumstance on which Marx places great weight is material abundance.) Being optimistic about the future, Marx cannot be as cynical about human motivation in the present as commentators often take him to be.*

In the middle of the seventeenth century an astute political scientist wrote, "Of all Voluntary Acts, the Object is to every man his own Good" (Hobbes, 1968, p. 209). If we treat this statement not as a tautology but as a broad empirical claim, we may render it so: each person always acts with the aim of satisfying narrowly self-interested goals. One might quibble about how to interpret "narrowly." How narrow is narrow? In the tradition of political analysis that was inaugurated or at least given a hefty push forward by Hobbes, acting from narrowly self-interested motives can include acting to benefit friends and family members whose interests have become incorporated into the self-interest of the agent. The crucial denial is that people ever willingly act to sacrifice "their own" interests (including the interests of those near and dear to them) for the sake of mere strangers or for the sake of a mere perception of moral duty. Call this claim the "assumption of egoism." Obviously for this assumption to do useful explanatory work, it need not be strictly true, just close to true. If self-sacrificing conduct motivated by altruism or devotion to morality sometimes occurs, but too infrequently and on too small a scale to alter political outcomes significantly, the simplifying assumption of egoism will suffice for political theory. The assumption of egoism figures in a venerable research program in political science that is very much alive and kicking.

As one of the great innovators and eccentrics in social theory, Karl Marx is hard to pigeonhole in terms of the category just described. Critics have seen in Marx both a penchant for generalizing social science invoking the assumption of egoism and an anticipation of hermeneutic and critical theory methods geared toward the normative interpretation of culture (e.g., Habermas, 1971, pp. 25-63; 1973, p. 238; and Gouldner, 1980). In this article I consider an interpretation and criticism of Marx that place him squarely in the Hobbesian camp. To defend Marx from the criticism, I argue, one must deny that Marx accepts the assumption of eogism (but this denial does not call in question Marx's commitment to generalizing social science).

Marx's theory of social change postulates that people are sometimes motivated to action by class interest, and that some of these actions crucially influence historical development. In the words of the *Communist Manifesto,* "The history of all hitherto existing society is the history of class struggles" (Marx & Engels, 1976b, p. 482). The upshot of Marxian social theory is that certain nasty features of capitalism render life unpleasant for capitalist workers. Workers can improve their group situation through collective action; in fact, as a group they would be better off if capitalism were abolished. Hence, the theory concludes, once workers comprehend their situation they will be disposed to undertake collective action to ameliorate their lot under capitalism and to do away with capitalism altogether when prospects are good that revolutionary action will succeed.

The premises of Marx's argument are open to

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The author is a member of the Department of Philosophy at the University of California at San Diego.

challenge at many points. In 1965 Mancur Olson objected to its logic (Olson, 1965/1971; see also Tullock, 1971, and especially Buchanan, 1982). On Marx's own premises, according to Olson, he ought to have concluded that although it is in the interest of the workers as a group to revolt against capitalism, such action is not in the individual interest of any worker, so that workers if rational will not revolt. In this article I rehearse the Olson criticism and show how Marx might have defended himself against it. For this project I accept two ground rules: 1) any view imputed to Marx must square reasonably well with his texts, and 2) to count as a *defense* of Marx, an interpretation of his thought must not only evade the criticism under discussion but also propound on his behalf a view that is at least *prima facie* plausible and worthy of further examination in the light of present social science knowledge.

## The Problem

Consider a good that is *collective* with respect to some group in the sense that if any person in the group consumes some of the good, it is not feasible to exclude any other group member from consuming some of it. Assume that the members of a group are *rationally self-interested* in the sense that each member of the group acts efficiently to maximize his own expected benefit. Consider a group of *N* persons for whom a particular good that might be produced is collective. A *feasible supplier* of this good for *N*, let us say, is any subgroup for whom the gains from supplying the good exceed the total cost of supplying it. If the smallest feasible supplier in *N* has so many members that any member's contribution toward provision of the good is imperceptible to other members and will not affect any other individual's decision whether or not to contribute, the group *N* is in Olson's terminology "large." No individual of a "large" group has a rational incentive to contribute toward provision of the collective benefit, according to familiar dominance reasoning.[1]

The application of this simple apparatus to Marx's theory is straightforward. According to Olson's Marx, socialist revolution—along with lesser benefits from smaller-scale class struggle—is a collective good for the class of workers, but the workers constitute a "large" group, so none has a rational incentive to make a contribution toward the collective cause.

To put the point in another terminology, as suggested by Russell Hardin (1982, pp. 25-32), Olson is saying that the logic of proletarian revolution mirrors the logic of a many-person single-play Prisoner's Dilemma (PD). Even though all would benefit from the hoped-for revolutionary action, none has good reason to initiate it, so the revolution will not occur unless revolutionaries behave irrationally. The notoriously observable absence of revolution in the industrially advanced countries, where Marx predicted revolution would occur, appears to be only one among many examples of the suboptimal voluntary provision of collective goods that neoclassic economic theory leads one to expect as a matter of course.

## First Solution: IIPD

In single-play PD, cooperation is not rational. But in indefinitely iterated prisoner's dilemma (IIPD), cooperation may be rational.[2] In this section I explore, and finally reject, the suggestion that the logic of many-person revolutionary class struggle should be modelled as the logic of many-person IIPD.

Iterating a PD opens up possibilities of rational cooperation, because each player must consider the possibility that what he does now may affect the behavior of others on future plays (see Luce & Raiffa, 1957, pp. 97-102). In an iterated PD, dominance reasoning does not support the strategy of "always defect." If a PD is to be iterated an unknown number of times, there can be coordination equilibria—strategy pairs such that no player would benefit from a change of strategy by any one player—provided certain conditions are met, chief among them 1) that each player's strategy dictates a punishing response to defections by others, and 2) that each player holds the probability of further repetitions of the game to be sufficiently high and the number of probable repetitions to be sufficiently large so that the long-run prospect of sustaining cooperation promises greater gain than the short-run temptation of defection.

Differences in the incentives facing agents in many-person as opposed to two-person iterated PD suggest that even if cooperation does occur in some many-person versions, it is likely to limp along at suboptimal levels. Roughly, the difference is that in two-person IIPD, punishing a non-cooperator directly benefits the punisher, whereas in many-person IIPD, cooperators cannot punish the defection of free-riders in subsequent play without harming themselves by denying the benefits of cooperation to all. Nonetheless IIPD analysis might well be the appropriate explanation of instances of ordinary or trade-union class struggle, brought about by labor-management conflicts of interest that are expected to recur into the indefinite future. "Ordinary" class struggle contrasts with "extraordinary" class struggle, the latter being marked by the expectation that a fundamental change in property relations throughout society is possible in the very short run and by a greatly increased willingness on all sides to break the customary rules of the ordinary class struggle game.[3] Even if ordinary class struggle should prove to be explicable on the model of self-interested rational behavior in many-person IIPD situations, extraordinary class struggle is Marx's abiding analytical concern and the linchpin of his doctrine.

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[1] Notice that in the extreme case where a collective benefit will not be obtained unless all members of a group contribute toward its provision, the incentive to take a free ride on the contributions of the others disappears, regardless of the size of the group. For a thorough explication of Olson (1965/1971), see Hardin (1982, chaps. 2-3).

[2] For analysis of PD repeated a fixed number of times, where the number of iterations is common knowledge to all participants, see Schick (1984, pp. 68-76).

An extraordinary class struggle is unlikely to form a pattern that is amenable to IIPD analysis. IIPD-style reasoning seems inapplicable to the moment of revolution, when the game being played is unique and nonrepeatable and known to be such by the players. To be sure, one can often analyze a single outbreak of revolt into a connected series of events each of which can be viewed as a single play of a game. But there are disanalogies between this scenario and IIPD. Strategic interactions that can be represented as IIPD often owe their character to the presence of stable institutions that guarantee that the game is likely to be repeated over and over, but such institutions may disappear or become problematic in the chaos of approaching revolution.

Mulling over the dynamics of street protests that occurred in St. Petersburg in February, 1917, Trotsky observed, "A revolutionary uprising that spreads over a number of days can develop victoriously only in case it ascends step by step, and scores one success after another. A pause in its growth is dangerous, a prolonged marking of time, fatal" (Trotsky, 1964, p. 117). Suppose Trotsky's conjecture holds. The revolutionary process then decomposes into a series of games each of which (after the first) occurs only if a sufficient level of cooperation has been attained in the preceding game. It is not known in advance how many games must be played to determine the success or failure of the revolutionary outbreak, but in the nature of the case the sum of payoffs for the complete series is pretty much fixed independently of how many games intervene between the present one and the final decisive encounter (what matters is, for example, not how many street demonstrations occur but whether or not they topple the regime). I take it to be plausible also to suppose that when the decisive game occurs, in which successful cooperation would ensure a successful revolution, the decisiveness of the encounter is common knowledge to all or at least presumed with high probability. But this last point destroys the IIPD-like appearance of the series.[4] If rational self-interested cooperation through the series is explicable, this can only be because the structure of payoffs in the last decisive game is *not* PD, rather assurance or some other matrix that renders cooperation individually rational. If this final encounter is PD, in anticipation of this sad ending would-be revolutionaries if rationally prudent will decline to join at the outset. Either way, IIPD analysis turns out to be not pertinent.

To put the same point another way, any positive joint payoff for mutual cooperation, compared to mutual defection, in the early encounters of a revolutionary crisis will just consist in an enhanced likelihood that the movement will not fizzle out before a decisive engagement in which winning the battle would mean winning the civil war. But if the assumption at the outset must be that in the end everything hinges on a single contest, then the dynamic of mobilizing revolutionary forces must be quite unlike the dynamics of building trade union support, where there can be joint positive payoff for cooperators at many points along a path that extends into the indefinite future.

Besides being essentially single-play, extraordinary class struggle differs from ordinary class struggle in the number of participants required to sustain it. Other things being equal, one would expect that the larger the number of players in a many-person IIPD, the smaller the prospect of cooperation, owing to rising costs of explicit or implicit bargaining, bluff, and strategy. When Marx writes of class struggle as a constant daily fact of capitalist society, he is thinking of small-scale conflicts pitting a few workers against a single capitalist. Despite the common label, the class struggle that in Marx's theory is expected to precipitate the demise of capitalism is a very different phenomenon, involving at least transnational common struggle by the working class of several European nations for transcendent aims.[5] The large number of participants involved in Marxian scenarios of revolution itself militates against the prospect that the possibility of cooperation among rational egoists in IIPD situations could explain how such a revolution came about, should one ever occur.

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[3] A taxonomy of varieties of extraordinary class struggle is beyond the scope of this essay. Extraordinary class struggle can be political (aimed at replacing the government or overthrowing the state) or social (aimed at wresting control of economic enterprises) or both.

[4] The example in the text concerns extraordinary class struggle directed toward a coup. Consider a revolution that follows the syndicalist pattern of a wave of enterprise occupations. Each occupation decomposes into a series of games, but if a single encounter is believed to be decisive, the point in the text holds here also. Or one could consider the players to be collectives of workers in a number of enterprises, and the question for each collective is whether to join in a wave of enterprise takeovers. This issue, if PD at all, is once again single-play PD.

Marxist usage of the term "class struggle" tends to accord this honorific label to small-scale disputes involving small numbers of workers arrayed against their bosses, even if a successful outcome of the struggle for those few workers would be disadvantageous to other members of the working class. Generosity in classifying fractional conflict as class struggle usually proceeds either from the hope or the prediction that fractional conflict will set in motion a process leading to more conflict on an increasingly wide scale and culminating in class struggle more strictly construed as struggle by the entire class or in the interest of the entire class. But insofar as IIPD-style cooperation is assumed to be the driving force of class struggle, the expectation that parochial or balkanized disputes could merge smoothly into unified revolutionary struggle looks to be misplaced.[6] The motivations that might ignite the one will not spread to the other. The two processes are apparently discontinuous.[7] From the fact that workers cooperate with each other in small-scale and geographically concentrated conflicts in which defection from cooperation can readily be detected and the threat by cooperators to cease cooperating if defectors do not pay their dues can be credible, a rationally self-interested person will not infer that it is reasonable to cooperate in large-scale and geographically dispersed interactions in which such detection is difficult and such threats are bluff.[8]

## Second Solution: Selective Incentives

Perhaps the best way to escape a PD trap is never to be caught inside one. Commentators have found several sources of selective incentives in situations that would otherwise be PD. These are holes in the trap. A brief review of the prospects for explaining the emergence of class struggle by these means is in order.

One idea is that cooperators might credibly threaten selectively to punish non-cooperators, thus diminishing the attractiveness of the free ride strategy. In this connection Olson mentions the violence and coercion that appear to be endemic to labor dispute in American history (Olson, 1971, pp. 66-76).[9] But a full explanation of how cooperation might emerge and become stable is unlikely to be forthcoming from this quarter. For so far we have not been informed how cooperation rises to the point at which there is a substantial body of cooperators who find themselves irked at the non-cooperating behavior of a few. Even if we assume an initial state of cooperation, with just a few defectors, the sanctioning of these defectors will be costly, and the gains to be had from sanctioning will be shared among all cooperators regardless of who joined the sanctioning posse. The collective action problem replicates itself at the level of organizing the imposition of sanctions.

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[5] As evidence of the immaturity of the French working class in 1848, Marx cites its failure to perceive the need for a European revolutionary war against England (Marx, 1978b, p. 56).

[6] For another argument supporting this claim, see Roemer (1979, pp. 763-767).

[7] Rosa Luxemburg aptly characterizes this discontinuity: "With the psychology of a trade unionist who will not stay off his work on May Day unless he is assured in advance of a definite amount of support in the event of his being victimised, neither revolution nor mass strike can be made. But in the storm of the revolutionary period even the proletarian is transformed from a provident *pater familias* demanding support, into a revolutionary romanticist for whom even the highest good, life itself, to say nothing of material well-being, possesses but little in comparison with the ideals of the struggle" (cited in Holmstrom, 1983, p. 315).

[8] Closely related to IIPD possibilities is the possibility of interdependent strategy choice. A situation that otherwise meets the conditions of single-play PD will still fail to be PD unless players' strategy choices are independent in the sense that no player is able to adopt a strategy that makes his choice of play contingent on what others do on that very same play. This is a strong assumption and one may wonder whether it holds true or even approximately true in real-world conflict and cooperation settings. For example, suppose that after a snowstorm neighbors have preferences regarding the shoveling of snow from their sidewalks that form a PD configuration. The situation will not be PD if each neighbor can remove the snow from his own walk while simultaneously watching to see that every other neighbor is doing the same. Is the formation of class struggle the soluble snow-shoveling problem writ large? Obviously, the greater the number of shovelers and the more geographically dispersed they are, the more difficult it will be to make one's own choice of play on a particular move contingent on the choices of others. And equally obviously, extraordinary class struggle involves large numbers of geographically dispersed agents (although monitoring technology can to some degree reduce the importance of geographic dispersal). Pending detailed analysis of individual cases, it does not seem that the Olson criticism will fail to apply to Marx's analysis owing to absence of independent strategy choice.

[9] Barrington Moore sensibly observes that in a revolutionary setting, "There is liable to be a good deal of bullying by militants to force laggards into line and sustain the appearance of solidarity" (Moore, 1978, p. 321). Buchanan rather broadly claims that Marx does not countenance the use of intimidation to motivate proletarians to join in struggle (Buchanan, 1982, p. 93).

Another idea is that initiators of cooperation will anticipate the creation of an ongoing organization that will channel a stream of profits to the original founders. Union organizers will become trade union officials; today's revolutionaries will be tomorrow's commissars. Individuals with entrepreneurial talent alert people to opportunities for lucrative collective action, are paid by those people to organize cooperation, and perhaps attract initial followers by promising them shares of the expected payments (Kavka, 1982, p. 457).

Whatever its merits, this entrepreneurial account of the origins of class struggle appears to be inconsistent with Marx's expectation that class struggle in capitalist society will promote an egalitarian society free of hierarchy—as the *Communist Manifesto* puts it, "an association, in which the free development of each is the condition of the free development of all" (Marx & Engels, 1976b, p. 506).[10] A more ground-level difficulty is that the account suggests motivations for organizers, but supplies no reason whatsoever for members of the target population to pay these organizers, join their incipient organizations, and initiate cooperation under their direction (Laver, 1980, pp. 204-208). Organizers and entrepreneurs of class struggle may promise selective benefits to potential supporters, but on the assumption that all parties to these discussions are individually self-interested, why should the masses believe that the fledgling trade unionists or revolutionaries out of power will keep their promises once installed in power? Bygones are bygones for a self-interested utility-maximizer.

A third selective incentive that might supply rational motivation for self-interested members of "large" groups to contribute toward provision of collective benefits is negative contribution costs. If the act of contributing is valued for its inherent pleasures, apart from further consequences, these pleasures might offset the gains from free-riding sufficiently to motivate contribution (Hirschman, 1982, p. 86). One cannot enjoy solidarity and sibling cameraderie by sitting on the side lines but only by joining in the fray. Trivially, the solidarity of participants is available only to participants, but even vicarious solidarity would tend to be inhibited by the awareness that one is shirking participation. However, doubts intrude. One may wonder whether in many cases these participation benefits correctly fit into the category of strictly self-interested satisfactions. Why does banter with one's mates on a protest parade seem more zestful than banter with the same mates in a pool hall? If the answer is that one especially values contributing to a good cause in concert with one's friends, then the desire that is being satisfied is mixed in character, partly moralized rather than fully self-interested. Moreover, if participation benefits only flow from collective action that has reached a certain threshold size, the PD problem recurs for individual contributions to the initial stages of cooperation below the threshold level (Buchanan, 1982, p. 95). Another limitation to the participation benefits explanation of collective action is that participation benefits often are ephemeral, but collective action ordinarily must be sustained and steady in order to be successful. Reiterated, the initially fascinating union meeting becomes a crushing bore.[11] If the collective goal can be reached in one exciting leap, all may be well, but if many leaps are required the pace slows to a dull trudge, and free-rider conduct beckons.

The fading-joy limitation is particularly pertinent to what I have called "ordinary" class struggle, while the threshold difficulty applies with special force to extraordinary or revolutionary class struggle.

Regarding all selective-incentive explanations of how collective action arises, one issue is whether the stipulated incentives really are sufficiently strong and prevalent to motivate contribu-

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But Marx would surely accept that a proletarian movement democratically organized may rightfully use informal analogues of conscription for military defense. I agree with Buchanan, however, that a movement that is sufficiently well organized to enjoy access to means of coercion has already solved its collective action problem, so coercion cannot explain how collective action begins.

[10] The inconsistency arises, of course, only if entrepreneurially minded revolutionaries are correct in their belief that they can parlay leadership in the struggle into power and privilege in its aftermath.

[11] In a June 6, 1983 *Los Angeles Times* interview, long-time radical activist Dorothy Healey offers a bemused perspective on the joys of participation: "I hate meetings . . . My life has been absolutely filled with meetings. That's what the Left is . . . one big meeting" (Horowitz, 1983, p. 7).

tions in the case at hand. Another issue for us is whether Marx's texts suggest that he himself thought that selective incentives explain the emergence of class struggle. Without going into this latter issue I will just mention that the answer seems to me to be that he did not.

## Marx on Motives

Participants in class struggle as Marx conceives it are motivated by a desire to promote their common class interest. This entails that the participant is willing, at least to some extent, to sacrifice his personal interest and even the interest of his fractional subgroup within the working class to the general interest of the international working class. Marx is optimistic that this wide cooperation will be forthcoming. In an interview with a journalist regarding the alleged conspiracies of the International Workingmen's Association, he comments, "Formerly, when a strike took place in one country it was defeated by the importation of workers from another. The International has nearly stopped all that. It receives information of the intended strike, it spreads that information among its members, who at once see that for them the seat of the struggle must be forbidden ground" (Marx, 1974, p. 395). The compulsion in Marx's "must" arises from a sentiment of caring for fellow members of one's class, not a perception of where one's individual self-interest lies. In a loftier tone, Marx observes, "It is one of the great purposes of the Association to make the workmen of different countries not only *feel* but *act* as brethren and comrades in the army of emancipation" (1974, p. 86). The members of this army strive to defeat the opposing forces of capital, but their aim is emancipation not for themselves alone but for humanity. To be motivated by class interest in the relevant sense is *not* to be disposed to promote the welfare of one's own class at the expense of the welfare of all other classes. Class interest is not supposed to induce proletarian fighters to wrest an advantage for their class at the expense of the desperately poor, chronically unemployed underclass, for instance. This is so despite Marx's judgment that the lumpenproletariat is more likely to furnish mercenaries of the established order than allies of the revolutionary cause. In short, the "interest" component of class interest becomes thoroughly attenuated under inspection. Fully class-conscious workers aim to promote the interests of the international working class because they subscribe to a theory (namely Marx's) according to which promoting working-class interests is the best available strategy for improving the future for humanity generally.

Marx's accounts of struggles in which the participants lack full class consciousness also refer to motives beyond egoism. In a well-known passage of *The Eighteenth Brumaire* (1979), Marx makes an invidious contrast between the French working class, capable of bold political action to defend its interest, and the French peasantry, incapable of such initiatives, at least in the historical period under review (1848-1851):

> The small peasants form a vast mass, the members of which live in similar conditions, but without entering into manifold relations with one another. Their mode of production isolates them from one another, instead of bringing them into mutual intercourse. The isolation is increased by France's bad means of communication and by the poverty of the peasants. Their field of production, the small holding, admits of no division of labour in its cultivation, no application of science, and, therefore, no multiplicity of development, no diversity of talents, no wealth of social relationships (Marx, 1979, p. 187).

Marx expects the reader to infer that, in contrast with the peasant's situation, large-scale industry does admit of extensive division of labor, the application of science, and so a wealth of social relationships among the industrial workers. Some have seen in this passage a glimmering of the idea that an anomic pattern of social relationships lacking repeated encounters among potential cooperators will tend to prevent the emergence of cooperation in PD situations (Hardin, 1982, p. 184). But the vision of working class politics that Marx contrasts to his sketch of peasant existence is too epic, too grand to be reducible to this sensible, mundane thought.

While mordantly cynical in some respects, Marx's description of clashes and battles in February-June 1848 is entirely uncynical in its presentation of the bold and self-sacrificing motivations of revolutionaries past and present. "But unheroic as bourgeois society is, yet it had need of heroism, of sacrifices, of terror, of civil war and of national battles to bring it into being" (Marx, 1979, p. 104). Marx observes that after the February revolution, "the Paris proletariat . . . revelled in the vision of the wide prospects that had opened before it" (p. 109). The dazzling view here alluded to is not merely one of more efficient means ready to hand for satisfying proletarians' narrow self-interests, but rather a perhaps utopian vision of a better world for all. This vision motivates collective action, even hopeless action, in the story Marx tells. Praising the heroism of the Paris proletariat in the street fighting of the June days, Marx comments, "At least it succumbs with the honors of the great, world-historic struggle" (p. 111). Generalizing about proletarian revolutions in the modern era, Marx observes that they "recoil ever and anon from the indefinite prodigiousness of their own aims, until the situation has been created which makes all turning back impossible" (p. 107). This does not mean: a situation is reached in which every revolutionary calculates that his expected personal payoff from the strategy of retreating or deserting the cause is less than the expected personal payoff from the strategy of going forward. Rather the idea is that a point is reached at which turning back would renege on a commitment to one's most ideal self-image, to be realized in the attainment of the most prodigious aims by heroic means. This is Marx the German Romantic, not the sober Victorian political economist.

Let's return to the peasant side of the worker/peasant comparison Marx is making. Notice that what Marx is specifically attempting to explain is not how workers are able to cooperate in collective action for common goals while peasants fail to act collectively. Rather he is trying to explain why the peasants' main political intervention was to vote for authoritarian rule in December, 1848, while workers had managed to mount sporadic active resistance to constituted authority between February and June. The act of casting a ballot for Louis Bonaparte may express the class viewpoint of the peasantry but is not—for all its lack of grandeur—in the individual self-interest of any given peasant (even on the assumption that Bonaparte's election would be in the collective interest of the peasantry). The problem Marx is trying to solve is simply not the problem Olson accuses Marx of failing to solve.

The same conclusion is suggested by Marx's emphatic statement that voting for Bonaparte was an intellectual error on the part of the peasants. The vote cast expressed their viewpoint, not their rational interests. As Marx puts it: "The Bonaparte dynasty represents not the revolutionary, but the conservative peasant . . . not the enlightenment, but the superstition of the peasant; not his judgment, but his prejudice . . ." (p. 188). The problem is not that the peasant's rational calculation of his individual self-interest inhibits him from acting with others to secure a collectively optimal outcome. The problem in Marx's jaundiced view is that the "rural idiocy" of peasant life precludes his taking an objective and enlightened view of his class interests. Whatever disposition to sacrifice self for class that peasants possess will come to naught as long as ignorance and other intellectual vices vitiate their perception. When Marx characterizes the French peasantry as unable to enforce its class interests, he is not denying the capacity of the peasants to initiate a jacquerie. He is denying that anything will come of such actions. His reason is that the peasantry lacks "manifold relations" and a "wealth of social relationships." I suggest putting a cognitive gloss on these phrases. The modern world is complex, but the benighted peasant does not encounter its complexity, whereas the industrial worker, whose social experience is more various, is more prone to adopt sophisticated social theories reflecting this modern complexity. So at any rate Marx hopes.

## Human Nature: Marx's View

If Marx does not hold that narrow self-interest supplies good reason for individual participation in class struggle, on what motivational assumptions does he then rely? If class struggle is the motor of history, what propels the motor?

The clue I propose to follow is to ask what view of human nature must be assumed in order to make sense of the millenarian aspect of Marx's thought—his assumption that whereas "(i)n all revolutions up to now the mode of activity always remained unscathed and it was only a question of a different distribution of this activity . . . the communist revolution is directed against the preceding *mode* of activity, does away with *labour,* and abolishes the rule of all classes with the classes themselves . . ." (Marx & Engels, 1976a, p. 52). For on the assumption that humans under capitalism are rational egoist utility-maximizers, Marx's optimistic expectation is utterly at odds with his own theory. The Marxian expectation I am querying is not the prediction that the proletariat will wage a successful revolution against capitalism; rather it is the prediction that postcapitalist society will be classless, egalitarian, tolerably free of injustice in basic social institutions. Society abhors a vacuum. If a ruling class is suddenly displaced from power, a remarkable opportunity presents itself for rationally self-interested agents to wrest power for themselves, to consolidate their advantage, and to extract profit and privilege for themselves from their positions of power. Only if *per impossible* each individual's power precisely and continuously balances the power of every other individual will rational egoists caught in the turmoil of revolution fail to move to an equilibrium recognizable as the formation of a new class society.

Any interpretation must reckon with Marx's failure to address this issue directly.[12] Occasion-

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[12] A sketchy but pertinent comment appears in *The German Ideology*: "Both for the production on a mass scale of this communist consciousness, and for the success of the cause itself, the alteration of men on a mass scale is necessary, an alteration which can only take place in a practical movement, in a *revolution*; the revolution is necessary, therefore, not only because the *ruling class* cannot be overthrown in any other way, but also because the class *overthrowing it* can only in a

ally Marx seems to hint that the distinguishing factor in the approaching working-class revolution is that for the first time it will not be a segment of society that takes power, but a homogeneous class comprising virtually all members of society. According to Marx a capitalist society ripe for revolution has a simplified class structure consisting of a tiny minority of capitalists and their allies surrounded by an undifferentiated mass of unskilled workers. After the revolution, in this scenario, society takes control of society. Society will have no stake in securing vested interests against itself. In the words of the *Communist Manifesto,* "All the preceding classes that got the upper hand, sought to fortify their already acquired status," but the proletarians "have nothing of their own to secure and to fortify" (Marx & Engels, 1976b, p. 495; see also p. 485). Despite appearances, this passage does not engage the problem just posed. Marx's claim rests on an extreme version of his class-polarization thesis, which has proven to be false, but even assuming its correctness for the sake of argument does not help with the difficulty. Assume a huge homogeneous proletariat united by the need to cohere in struggle to oust the capitalists: once this struggle is successful one should expect to see this proletariat rapidly become heterogeneous as individuals scramble to do as well for themselves as they can by fishing in the troubled waters. Having nothing of his own, a rational egoist will have all the greater motivation to grab something that will become his own to secure and to fortify.

I submit that the problem of rendering intelligible Marx's reasons for optimism about the future of society will prove intractable unless we are willing to jettison the assumption that Marx is assuming that individuals are always motivated solely by narrow self-interest. The explanation of why Marx fails to elucidate the connection between his cynicism about human nature and his utopianism about the future of society is simply that Marx does not embrace cynicism, so there is no connection to be elaborated.

An alert reader might well object that Marx believes that human nature changes and develops throughout history, so it is not inconsistent for Marx to suppose the individuals formed in capitalist society will be rational egoists while individuals formed in postcapitalist society will have quite other motivations. Indeed it is the process of class struggle itself culminating in revolution that transforms individuals or enables them to transform themselves from selfish individuals into socially minded citizens.

It is certainly true that Marx holds that human nature changes in important respects throughout history. The hunger of a savage differs from the hunger of a civilized gourmet (Marx, 1970, p. 147). *The German Ideology* states emphatically if somewhat vaguely, "The difference between the individual as a person and whatever is extraneous to him is not a conceptual difference but a historical fact" (Marx & Engels, 1976a, p. 81). But from the fact that Marx believes that human nature is changeable in some respects it does not follow that it would be inconsistent for Marx to claim that in some respects human nature is unchanging (Cohen, 1978, p. 151, makes this point).

The claim that Marx believes that human beings are nasty (egoistic) in class society and become nice (altruistic) in the transition to classless society, I will call the "standard view." I have urged that if the standard view is correct, then Marx is after all vulnerable to the Olson criticism, for he provides no coherent account of how purely self-interested persons can unite to overthrow capitalism, much less inaugurate communism. Nor does Marx offer even a rudimentary beginning of an account of how, on the assumption that capitalism is eliminated and communist institutions are installed, these institutions will work to alter human nature away from egoism and toward communalism. To me these considerations suggest that we should reject the standard view if we can elaborate an acceptable alternative, and the alternative I propose is that Marx believes that a concern for others, a moral concern, is a stable component of human motivation throughout history, but one that is all but submerged by adverse circumstances that persist invariant up to the demise of capitalism.

Before adumbrating this "moralized" view of Marx's beliefs about human nature, a preliminary and apparently decisive difficulty must be faced. My view attracts the obvious objection that Marx frequently expresses a tough-minded cynicism about human nature and shows great ingenuity in ferreting out possible selfish motives for what superficially seems to be philanthropic and idealistic action. Any interpretation such as mine that denies that Marx loudly laments the universal bourgeois egoism that capitalist institutions

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revolution succeed in ridding itself of all the muck of ages and become fitted to found society anew" (Marx & Engels, 1976a, p. 53). Marx's idea here is that participation in revolution is ennobling. This is plausible, but so is Brecht's frequent theme that participation in revolutionary activity hardens and coarsens. Even supposing that Marx rather than Brecht is correct on this point, there will be a wide range of individual degrees of involvement in revolutionary activity, and in any reasonable scenario a majority of the population will not be actively involved to a significant extent: hence they will not fully undergo the revolutionary character transformation that Marx says is crucial.

engender thereby refutes itself—so goes the objection.

To illustrate how this objection can be defused, I will examine a typically cynical comment by Marx and Engels that happens to be cited by Olson in support of the standard view, of which he is himself an adherent. This famous passage from the *Communist Manifesto* runs as follows: "The bourgeoisie has left remaining no other nexus between man and man than naked self-interest, than callous 'cash-payment.' It has drowned the most heavenly ecstasies of religious fervor, of chivalrous enthusiasm, of philistine sentimentalism, in the icy water of egotistical calculation" (cited in Olson, 1971, p. 104). To read this passage correctly one must distinguish between 1) an ideology that describes and rationalizes ordinary or normal social behavior, 2) people's actual ordinary social behavior, and 3) the full range of people's desires only some of which are expressed in ordinary social behavior. The passage quoted just above deals with 1) and 2), not 3). Marx and Engels are saying that capitalist ideology predicts and prescribes narrowly self-centered behavior, whereas feudal ideology prescribed and predicted unselfish behavior in conformity with the norms of religion, chivalry, and so on. Actual behavior under both feudalism and capitalism was for the most part narrowly self-centered. To this extent capitalist ideology is more accurate and less hypocritical than its feudal counterpart. But to say this is not to pronounce at all on the topic of 3). On this last point the passage quoted is strictly noncommittal. In fact it is fully compatible with the further assertion that most people's strongest preference is to cooperate with others on fair terms, but people inchoately recognize that the going terms of actual social cooperation are fixed beyond their control, so the preference they mainly act upon is to do as well for themselves as they can rather than play the role of sucker in a dog-eat-dog world. This indeed is the gist of the position Marx holds on my interpretation. I don't say the quoted passage requires this further gloss, only that the two are fully consistent. Pointing to Marx's cynical quips does not begin to settle the interpretive issue.

The thought I am relying on here is straightforward: people's actions are determined by their desires in conjunction with their beliefs, so even a very strong desire coupled with a belief that the desire is unrealizable may not show itself directly in behavior. Of course, if Marx or anybody else does wish to advance the hypothesis of a latent preference, he is obliged to provide empirical evidence, perhaps of an indirect sort, to the effect that the latent preference postulated is really present and not merely the social theorist's fantasy. Marx's remarks on ideology do supply materials from which one could begin to construct such a case. People's proclivity to accept ideologically biased theories of fairness and, I would add, sometimes to act on their ideology even against their own perceived interests, is plausibly interpreted as reflecting a powerful psychological pressure to think that one is on the whole behaving fairly where this pressure is blocked by dim recognition that one's acceptance of genuinely fair shares would entail intolerable personal sacrifice. My object here is not to marshall evidence but to mark how one might do so on Marx's behalf and to note that Marx is properly sensitive to the hazards of putting forward unfalsifiable claims about human nature.

Setting the issue of evidence aside, I want to take as a clue to Marx's core beliefs about motivation his thought that an egalitarian society is possible only on the basis of the enormous wealth that is accumulated under capitalism. That increased wealth enhances prospects for egalitarianism is explicable only if one assumes that Marx ascribes to human nature a set of motives or dispositional traits roughly along the following lines:

> 1) Persons strongly desire a commodious existence.
> 2) Persons desire to cooperate with others on fair (equal) terms.

At least two phrases in these statements are subject to variation in meaning owing to the varying pressure of prevalent social ideology: "commodious existence" and "fair terms." But here I leave aside the complications that would have to be introduced into this account in order to incorporate Marx's ideas on ideology.

Under conditions of scarcity that according to Marx have characterized all of human history down to the mid-nineteenth century, cooperation on fair (equal) terms among all members of society would have been incompatible with the enjoyment of a commodious existence by any member of society. In this sense 1) and 2) conflict. People have tended to resolve this conflict in their impulses by restricting—or acquiescing in the restriction of—the scope of their desire to cooperate: the disposition to cooperate on terms perceived to be fair is normally effective only within groups, such as family, clan, caste, class, race, nationality, with which the individual identifies herself to a greater or lesser extent. Being gregarious, people for the most part do not behave as egoists. Being less than thoroughly altruistic, people for the most part confine their concern for others within the boundaries of salient group identity. Marx's particular emphasis is on the salience of economic class identity. Summarizing, we have:

> 3) When cooperation with all on equal terms precludes commodious existence for any, people will tend to confine their willingness to cooperate to their group memberships—among which economic class looms large.

Attributing 1)-3) to Marx interprets him as holding that class oppression is necessary for social order throughout human history up to the point when capitalist economic abundance creates the possibility of a commodious existence for all. By "class oppression" I mean a division of the benefits of social cooperation among classes that is both unfair and coercively enforced. When there is not enough to go around, people will cluster in groups that struggle for a larger-than-average share. Class oppression is "necessary" for social order in the sense that social order is brought about by it and could not exist without it. To see that class oppression creates and sustains social order, it suffices to note that an egalitarian division of benefits in a nonaffluent society would be a standing temptation, greater than human nature could bear, to violent social conflict and civil war. Oppressive social order is of course unstable at times, but an egalitarian social order yielding a pinched and cramped standard of living for all is necessarily unstable, given human psychology as Marx perceives it. Provision of a commodious existence to some engenders loyalty to the providing regime, and a regime cannot survive unless it elicits such loyalty. But provision of a commodious existence to a privileged section of society is tantamount to class oppression.

G.A. Cohen has recently denied that class oppression is instrumentally necessary to social order throughout human history down to the time of capitalist abundance (Cohen, 1978, pp. 207-213). (He also denies that Marx's texts show whether or not Marx himself held this view.) In support of this denial, Cohen points out that there have been stable precapitalist oppressive societies in which a ruling elite extracts a surplus from a peasantry that is self-directing in its economic production and seems to receive no favors from the elite. It might be thought that in such circumstances the taxing or pillaging ruling elite does not, merely by its oppression, contribute anything at all to the maintenance of social order. Although plausible, such an inference would be mistaken. By filling the top position of privilege in a hierarchy, the ruling elite preempts the fierce conflict for that top slot that would prevail in its absence. As Marx famously puts it, the overcoming of alienation requires a high development of productive forces because, in the absence of abundant wealth, equality of shares means only that "*want* is merely made general, and with want the struggle for necessities would begin again, and all the old filthy business would necessarily be restored" (Marx & Engels, 1976, p. 49).

Taken by themselves, 1)-3) do not say what will happen in the situation of economic abundance that in Marx's estimation is capitalism's signal achievement. In a capitalist society, the desire for a commodious existence and the desire to cooperate with all members of society on equal terms are, in principle, both satisfiable simultaneously. With a lessening of the conflict between 1) and 2), the tendency to parochial restriction of the willingness to cooperate begins to weaken.

> 4) When 1) and 2) are jointly satisfiable for all members of society, individuals tend to develop a desire to cooperate on an increasingly wide basis, with all others who are willing to reciprocate cooperation on the same equal terms, ultimately with humanity generally.

Of course this impulse to harmony is all but stifled: "The tradition of all the dead generations weighs like a nightmare upon the brain of the living" (Marx, 1979, p. 103). Marx singles out two complementary inhibiting factors.

> 5) Once having acquired power and privilege far above the average, individuals will not voluntarily relinquish the positions of advantage that confer these extraordinary benefits. (In short, power corrupts.)

According to Marx, besides the resistance of the privileged capitalist class, the other solid obstacle blocking the development of postcapitalist harmony is the passivity of the underprivileged class.

> 6) Low social status, lack of experience in getting one's way in the world, and denial of the means of commodious existence when these means are not objectively in scarce supply, all tend to inhibit people's willingness to enlarge the scope of the "others" whom they see as potential partners in reciprocal cooperation. Contrariwise, success in the pursuit of advantages by the disadvantaged will tend to break down this inhibition against reciprocal altruism. (In short, powerlessness corrupts.)

Poor conditions force people to struggle narrowly for their self-interest and, especially insofar as their struggles meet with frustration, persuade them that they are not just poor but poor people. In contrast, successful class struggle educates people toward desirable character traits.[13] Or at least there is an other-things-being-equal tendency in this direction, which, as Marx acknowledges, many adverse factors can deflect.

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[13] A recurrent theme of revolutionary analysts of Marxian persuasion is that the experience of small-scale

I am conjecturing that Marx is committed to claims somewhat along the lines of 4) through 6). I have no direct textual evidence for this attribution. Moreover, 4)-6) as they stand are too baldly stated. Contrary to what 5) seems to assert, it is not true that no king ever voluntarily abdicates the throne. It is not part of my present purpose to introduce the exact hedges and qualifications that would be needed to render 4)-6) weak enough to be invulnerable to obvious historical counter-example yet strong enough to support Marx's theory of proletarian revolution. What I do want to claim is that assumptions close to 4)-6) must be in the back of Marx's mind when he asserts that all history is the history of class struggle, that proletarian class struggle will produce a desirable end to class struggle, that voluntary resignation of power by a capitalist ruling elite is not a possible alternative route to this desirable end of class struggle, and that the process of proletarian class struggle will not be inimical to the widespread development of the citizenly virtues that will be needed in the commonwealth that is to arise in the final aftermath of class struggle. I want to claim that if one is to interpret Marx as espousing or at least gesturing toward a coherent theory, suitably refined versions of 4) to 6) will be essential parts of it.

The general level of wealth in society obviously affects the prospects for successful class struggle. Spartacus in ancient Rome did not have a chance, but the modern proletarian has a more realistic basis for hope. The greater the wealth at the disposal of the ruling class, the greater the incentive to make concessions to underclass demands, and such concessions inaugurate a cycle of increasing proletarian self-esteem, self-confidence, and widening cooperation.[14] Wealth does not guarantee a successful end to this cycle, but wealth's absence guarantees its failure: "Right can never be higher than the economic structure of society and its cultural development conditioned thereby" (Marx, 1978a, p. 53).

The thumbnail psychology sketched so far does not specify the exact strength of the motives it postulates. In this vagueness the sketch is true to Marx's thought. To round out the sketch, some account of ordinary versus extraordinary motivation is wanted. According to Marx, a time of social crisis and revolutionary upheaval can elicit unstinting heroism from people who do not in ordinary times give any indication of this potential for self-sacrifice. What explains this?[15] Marx believes:

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and localized conflicts can increase the participants' willingness to join in the very different activities of extraordinary class struggle. For example, Rosa Luxemburg writes, "Political and economic strikes, mass strikes and partial strikes, demonstrative strikes and fighting strikes . . . all these run through one another, run side by side, cross one another, flow in and over one another" (Luxemburg, 1925, p. 37). This would make no sense if the idea were that the practise of conditional cooperation by rational egoists in IIPD situations will increase the willingness of any egoist to cooperate non-egoistically where IIPD conditions do not obtain. But it can make sense to suggest that witnessing altruistic or morally motivated behavior on a small scale can enlarge people's sense of possibilities, disabuse them of ultra-cynical beliefs about the motivations of others, and thus increase their disposition to take risky initial steps toward large-scale cooperation (the success of which can in turn stimulate local protest activity). Cooperation with neighbors can induce one to cooperate with strangers who are also cooperating with their neighbors, but only if the basis of neighborly cooperation is not I'll-scratch-your-back-if-you-scratch-mine, for this arrangement cannot be extrapolated to cooperation with strangers at a distance, not at any rate in the range of cases of concern to Marx.

[14] Against Marx's view, Lipset marshalls some evidence for the claim that the granting of substantial concessions including "full political and economic citizenship" to the working class dampens its radicalism (Lipset, 1982, p. 2).

[15] John Roemer has an explanation that is very different from the one I offer (Roemer, 1978, 1979). He denies—and I concur—that IIPD reasoning is a very likely candidate for explaining "convulsive action" such as mass strikes. His further description almost suggests that convulsive action is irrational; the crowd is seized with madness. "The participants act, in a sense, despite their better judgment. They do not calculate rationally whether or not to strike, walk out, or fight the police. People are pushed, they retreat, they seek individual solutions; they are pushed farther, conditions become intolerable, some incident occurs, and there is an eruption" (1979, p. 763). No doubt workers like other people sometimes behave irrationally, but explanations of collective action on this basis will never vindicate Marx, who predicts that workers will engage in convulsive class struggle *and* that they will have *good reason* for doing so. Rather differently, Roemer also suggests that collective action proceeds not from irrationality but from a switch from individual to collective rationality. He doesn't elaborate, but I think he means that workers identify with their own class and act for its good rather than their own. But I have pointed out that the group identification in class interest is, in Marx's account, progressively generalized and moralized. Fully class-conscious workers act for the good of humanity. A revolutionary time arises precisely when ordinary people begin thinking self-consciously about the role they might play in world history, like Paris Communards. Roemer (1978) has a further interesting suggestion. Insofar as they perceive themselves as "participants in a highly social production process who operate under

> 7) Extreme situations can elicit extremes of behavior; in particular, a revolutionary social crisis can elicit extremes of heroism (as well as of dastardly conduct).

In a time of crisis, the possible gains and losses from bold collective action or its absence are greater than in normal times. Under favorable crisis circumstances, the expected gain to the collective from one's individual participation, discounted by the probability that one's action will bring about the desired outcome, may be spectacular. Hence, whatever altruistic willingness one has to sacrifice oneself for the collective will be far more likely to express itself in action at such times. The cautiously prudent person who becomes a bold revolutionary need not have undergone any drastic character change. Rather what has changed is the ratio of collective benefits to individual costs consequent upon bold action. Similar considerations explain how a prudent, seemingly virtuous person may give vent to extreme vice in a time of turmoil. (See Barry, 1970, pp. 30-32.)

The truth of 1)-6) all at once would not support the conclusion that it is ever rational for any individual person to revolt against capitalism. For 1)-6) at most warrant inferences about what people will want, not about what they will do. Desiring to cooperate with others on a reciprocal basis, I may yet reasonably refuse to cooperate if I lack good reason to believe others will reciprocate. Many persons could be in this position: all of us want to cooperate and none of us believes a sufficient number of us want to cooperate. The intensity of this problem increases as the number required for cooperation increases, because establishing the trustworthiness of strangers is difficult, so the problem will be severe for the emergence of class struggle. A further obvious component of the problem is that those who stand to lose if class struggle succeeds will have an incentive to, as it were, snip the telegraph wires that threaten to carry information that would promote mutual trust among wary class members. Moreover, one may suspect that elements of ordinary working-class culture will tend to inhibit growth of trust. If the individual harbors moralized desires that strike him as utopian, the resultant dissonance may lead him to want to deny their existence even to himself. A public display of cynicism that aims to persuade the agent himself may be the upshot of this dynamic and may in turn reinforce a similar dynamic in others. Finally, beyond the ignorance *that* others are willing to cooperate, class struggle may also be blocked by ignorance of *how* others are willing to cooperate. People may agree to man the barricades but be unable to agree on which barricades to man, and in this case, too, revolutionary action will fizzle.

The problem of misinformation and mutual distrust, though formidable, hardly seems intractable; it is essentially the difficulty of achieving a coordination equilibrium in a large-number assurance game with multiple coordination equilibria. This problem is helped toward solution by growth in mutual knowledge of players' preferences and strategy choices, whereas a PD situation is only entrenched by mutual knowledge of strategy choices.[16] The problem is rendered less formidable if people's disposition to cooperate includes a willingness to go the first mile rather than merely to reciprocate cooperation by others. We can distinguish a reciprocal and an anticipatory cooperator: the former is disposed to cooperate if and only if he believes others will cooperate also; the latter is disposed to cooperate on the first play (at least where the cost of initial cooperation is not excessive and there is no conclusive reason to believe others will not cooperate) and to continue cooperating on subsequent plays if sufficient others have cooperated on the previous move. In the absence of 1) any initial evidence of a widespread cooperative spirit and 2) excessive first-play cooperation costs, a population of anticipatory cooperators will succeed in establishing cooperation where a population of reciprocators would fail. In this sense a revolution may require the "faith that moves mountains."

## Conclusion

The human nature assumptions I have ascribed to Marx are very far from constituting a testable

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completely binding constraints" (p. 155), workers do not think in terms of optimizing choice among a range of possible strategies. But this suggestion is wrong. Capitalism may severely restrict workers' freedom but could not entirely eliminate it—even a slave has options. Nor does Roemer's association of a "no choice" mentality with the psychology of convulsive action seem plausible. In time of upheaval worker-radicals debate strategic choices with gusto. Exhilarating political argument widely shared is an important component of the sense of liberation experienced in periods of social turmoil.

[16] More accurately: strategy choices here may become interdependent if, for example, information about players' strategy choices becomes available serially. If one player announces his intention to meet others at the czar's palace, the others will follow suit even though many other meeting places were equally eligible before this announcement. (What looks like revolutionary authoritarianism may sometimes be a way of marking as salient one solution to a coordination problem.)

theory. At best these loose assertions form a vision or proto-theory from which a theory might be constructed. But hardly any progress has been made in producing nontrivial theories of human motivation from a nonegoist starting point, so in this respect Marx's analysis has not been superseded. The assumption of rational egoism that he eschews, despite its clarity and elegance, is at variance with established facts of political behavior that any political theory must comprehend. There is spadework yet to be done on the conceptual territory Marx begins to survey.

The doctrine that I have put in Marx's mouth says only that people have some disposition to be moral, where being moral includes cooperating in single-play PD situations, that this disposition is inhibited by material scarcity, which tends to increase the incentive for violating moral norms, and that capitalism for all practical purposes eliminates the necessity of material scarcity. Marx is a moderate millenarian. But the doctrine, if weak, is far from trivial: it denies that human dispositions are indefinitely malleable at the hands of the social environment. When there is not enough to go around, people will fight to advantage themselves over others, and class-divided society inexorably follows. When there is enough to go around, an egalitarian society is feasible, in the sense that its coming would make no improbable demands on human nature, and the measure of "enough" to go around is the per capita income of England after the middle of the nineteenth century. My characterization of Marx's thought as "millenarian" is not intended to carry any pejorative stigma. Marx is not alone, nor obviously wrong-headed, in resting his normative theory on secular faith that human nature permits the possibility of wide social cooperation to bring about a just or egalitarian society. But the promissory notes in the writings of Marx and others of his ilk who take a "soft" line on human nature have yet to be converted into cash.[17]

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[17] If, as I urge, introducing moralized motivation can save Marx's theory from the charge of incoherence levelled by Olson, it is otiose to deny that Marx's writings are amenable to interpretation in terms of theories of individual rational choice along the lines of expected utility maximization. Holmstrom (1983) asserts that Marx does and Marxists should reject the expected utility approach in favor of a new socialist theory of rationality, but she says very little about what such a theory of rationality might amount to. Nor, it should be added, does Marx even implicitly commit himself to criticism of a time-bound bourgeois theory of rational choice. If I am right, there is no specifically Marxist motivation to cast about for any such criticism or theory. Gary Becker writes, "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (Becker, 1976, p. 4). On my account, the Marxist will want to flinch and relent by postulating stable moralized alongside stable egoist preferences (or equivalently by postulating moral inhibitions that can constrain maximizing behavior).

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# Althusser's Marxism without a Knowing Subject

STEVEN B. SMITH
*Yale University*

*The quest for unassailable "foundations" for knowledge has preoccupied Western thinkers at least since Descartes. Without some such foundation or Archimedian standpoint, it was argued, our knowledge of the external world as well as our basis for moral and political judgment would fall prey to relativism, historicism, and ultimately nihilism. Recently, though, this Cartesian quest for foundations has come under attack from some of the most powerful minds of our age.*

*In this article I examine the contribution of Louis Althusser to this current of thought and assess whether his critique of foundationalist epistemologies and ethics can avoid the pitfalls of relativism. Althusser is compared to other thinkers who share his anti-Cartesian persuasion (Wittgenstein, Heidegger, and Foucault) and is criticized for his use of a kind of structuralism to abolish the "knowing subject" as the locus of thought and action. I conclude that Althusser's "antihumanism" has produced a convenient ideology for a new class of Marxist intellectuals to exert their claims to power over ordinary human agents who have been reduced to "bearers" or "supports" of certain systemic, structural relations.*

The title of this article is drawn from Karl Popper's 1967 lecture "Epistemology Without a Knowing Subject." Here we read:

> We may distinguish the following three worlds or universes: first the world of physical objects or physical states; secondly, the world of states of consciousness, or of mental states, or perhaps of behavioral dispositions to act; and thirdly, the world of objective contents of thought, especially of scientific and poetic thoughts and works of art. (Popper, 1972, p. 106)

It is not my purpose here to examine Popper's theory of the "three worlds" of knowledge or even to compare Popper's theory of scientific method to that of Althusser, although such a comparison might not be unfruitful. I want instead to examine Althusser's own attempt to produce a theory of the products of the human mind, what Popper calls those "unembodied world-three objects" apart from and independent of the "knowing subject."[1]

The attempt to eliminate the subject from the product of knowledge may well strike the reader as perverse. Ever since Descartes it has been the aim of modern philosophy to discover some "foundation" or Archimedian standpoint on which we can ground knowledge (Bernstein, 1983; Rorty, 1979). Virtually the textbook case of this approach is to be found in the opening pages of Descartes's *Meditations,* in which he tells of his efforts to find a secure foundation for knowledge in the "thinking subject" (*ego cogitans*) reflecting upon itself. Indeed, the radical challenge of Descartes was his claim that not tradition, not prejudice, not received opinion, but only the ego's own critical rationality can provide a touchstone for what is to count as knowledge. Although few philosophers today accept the substance of Descartes's argument, his search for some absolute, self-evident, and unimpeachable starting point for knowledge has captured the imaginations of both empiricists and rationalists alike. Without such a foundation, it is argued, our knowledge of the external world, as well as our basis for moral judgment and action, would fall prey to some form of relativism, historicism, or even worse.

And yet, Descartes's emphasis on the primacy of the knowing subject has not gone unchallenged. In opposition to the Cartesian paradigm with its image of the solitary thinker reflecting upon the possibility of knowledge, there has grown a powerful anti-Cartesian counter move-

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[1] The main texts consulted are Althusser *For Marx* (1969), *Reading Capital* (1970), *Lenin and Philosophy and Other Essays* (1971), *Politics and History: Montesquieu, Rousseau, Hegel, and Marx* (1972), and *Essays in Self-Criticism* (1976). To conserve space, I refer to these works by initials; for example, *For Marx* is cited as *FM.*

ment which has sought to "decenter" the privileged position of the subject. Hegel was perhaps the first to challenge this conception of the autonomous thinker by showing that reason itself has an intrinsically social and historical character. The knower is not apart from, but embedded in, the stream of history that he seeks to comprehend. The search for an Archimedian point for knowledge was pushed even further back owing to the progressive onslaughts of Marx, Nietzsche, and Freud, all of whom, in very different ways, sought to dethrone the autonomy of reason, showing it to be determined by certain subrational or extrarational forces that it failed to comprehend and control. In our own day the works of Lévi-Strauss, Foucault, and Althusser have further contributed to the "end" or "death" of man by showing that the subject cannot be analyzed apart from the overarching structures or systemic relationships in which it finds itself. The Cartesian image of the autonomous self reflecting in splendid isolation has been declared nothing more than a myth in which Western thought has been imprisoned for over three centuries.

This leads us back, then, to a consideration of the possibility of an epistemology without a knowing subject. For whatever we have heard lately about the advent of a "post-structuralist," a "post-Wittgensteinian," or a "post-Heideggerian" age, it is by no means clear that the Cartesian approach to knowledge is simply dead. In our century it has reappeared under the guise of Husserl's "transcendental phenomenology," Sartre's "existential ontology," and more recently in the various "possessive neo-individualist" theories of politics and society (Dallmayr, 1981, pp. 12-16). The works of Rawls, Nozick, and Dworkin all presuppose some Cartesian understanding of the self as unencumbered by any natural or empirically acquired ends or purposes as the only intelligible locus for their theories of rights and justice.

The question, then, is: To what extent can the search for an Archimedian standpoint for knowledge be abandoned and at the same time the pitfalls of relativism, historicism, and ultimately nihilism be avoided? If the Cartesian search for a certain and reliable foundation for knowledge and morality is indeed a myth, it does not follow that the rejection of foundationalism *tout court* will provide us with anything better. In this article I hope to show that a middle position between foundationalism and Althusser's anti-Cartesianism may therefore be defensible. Such a position may help to avoid the difficulties that Althusser's radical antifoundationalism raises for political thought and practice. As I shall argue, the attack upon foundations has simultaneously been an attack upon some of the major concepts and categories of the western liberal tradition, such as freedom, autonomy, dignity, and rights. It may be that what is at stake in this seemingly abstruse epistemological debate is the very survival of the humanistic tradition of political thought in general and liberal values in particular.

## The Banishment of the Author

Althusser's first assault on the subject occurs in his theory of reading and interpretation. No reading, he has maintained, is "innocent," by which he means that no reading can avoid making certain assumptions and presuppositions that prejudice the reader in a certain way (*RC*, pp. 14-15; cf. pp. 74-78). No interpretation, then, can be entirely neutral or value-free. Althusser's own reading is premised on the assumption that behind the "explicit discourse" of a text, there is a second "silent discourse," the meaning of which is "unconscious" to its author and must therefore be "dragged up from the depths." It becomes necessary then to abandon anything like the author's intention as providing a touchstone for interpretive validity and to focus on that esoteric or "symptomatic" discourse which must be examined for the "absences," "lacunae," and "silences" that the first conceals.[2]

Althusser launches a powerful attack on the thesis that the interpretation of a text must attempt to recapture an author's meaning in the manner he intended it to be taken. The main presuppositions of this kind of hermeneutic recovery are first, that the author is the sole determiner of the text's meaning; second, that this meaning is in principle objective, reproducible, and accessible to the interpreter who may be reading it from a different tradition or point in time; and third, that the meaning of the text is worth recovering. Such an understanding of the interpreter's art has a long history of its own, going as far back as Spinoza's attempt to recapture the literal meaning of Scriptures, but which more recently has been identified with writers as different as Leo Strauss, Eric Hirsch, and Quentin Skinner.[3] Whatever the differences between them, they all maintain that the primary goal of interpretation is to understand the meaning of a text through the recovery of the author's intention and his understanding of his own situation which requires, so far as possible, the elimination of prejudices and assumptions arising from the interpreter's own historical horizon. Indeed, in one currently important version of this approach, the interpretation of a text in the past is likened to the understanding of "speech acts" in the present.[4] The task of the interpreter is to determine "what [the] author, in writing at the time he did write for the audience he intended to address, could in practice have been intending to communicate" (Skinner, 1969, p. 49). It is clear from this remark that temporal distance or alien modes of thought should present no special difficulty to the interpreter trying to capture "what the author . . . could have been intending to communicate." All that is necessary is a knowledge of the prevailing "linguistic context" which determines the "range of possibilities" with which an utterance could have been conventionally uttered.[5]

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[2] This is not to be confused with the distinction between esoteric and exoteric writing made famous by Strauss (1952, pp. 22-37); see also Strauss (1959, pp. 221-232).

[3] A useful summary is provided by Gunnell (1979).

In contrast to the hermeneutic recovery of meaning, however, Althusser proposes a second, quite different style of reading "with nothing in common with the first" (*RC,* p. 24). This style of reading has nothing to do with understanding a writer in the way that he understood himself, but attempts to go beyond the author's own self-understanding to the "unconscious" presuppositions by which his thought was determined. These unconscious presuppositions Althusser calls the "problematic" of a work or text. This term is taken by him to signify a hierarchical structure of problems which sets internal limitations on what an author can and cannot say. Put in this way, the problematic serves as a kind of ideational infrastructure to the history of ideas in the sense that it determines what kind of problems may be posed as well as what kind of solutions to these problems may be found acceptable. The problematic may serve, then, to mark off the conceptual boundaries that identify the thinking of a writer, a school, or an entire tradition of thought. In this respect the problematic resembles, as Jameson has noted, nothing so much as Collingwood's theory of "absolute presuppositions" particularly in its idealistic character (Jameson, 1972, p. 137).[6]

The importance of the problematic as an interpretive device has, of course, been widely discussed in a variety of disciplines ranging from literary criticism to the history and philosophy of science. Readers familiar with this literature will recognize a family resemblance to Thomas Kuhn's theory of paradigms or conceptual models that govern the thinking of a scientific community.[7] The theory of paradigms has been adapted to the study of the history of political thought by Pocock, who has argued that the "linguistic paradigm" of a writer ought to take precedence over the author's professed intention, which can almost be, as it were, deduced from this paradigm. Taking exception to Skinner's appropriation of Austin's speech act theory, Pocock (1971) writes:

> Once history is seen in linguistic depth, the paradigm with which the author operates takes precedence over questions of his "intentions" or the "illocutionary force" of his utterance, for only after we have understood what means he had of saying anything can we understand what he meant to say, what he succeeded in saying, what he was taken to have said, or what effects his utterance had in modifying or transforming the existing paradigm structures. (p. 25)

Like Pocock, Althusser accords interpretive priority to the study of language "structures" over individual utterances or even entire texts. The problematic is, according to him, "the particular unity of a theoretical formation" (*FM,* p. 32). It is the overall framework of a theory which puts the basic concepts into relation with one another, determines the nature of each concept by its place and function within the whole, and confers on each concept its pecular significance. It determines the form or "horizon" within which problems can be posed as well as solutions entertained.

> This introduces us to a fact peculiar to the very existence of science: it can only pose problems on the terrain within the horizon of a definite theoretical structure, its problematic, which constitutes its absolute and definite conditions of possibility, and hence the absolute determination of *the forms in which all problems must be posed,* at any given moment in the science. (*RC,* p. 25)

The problematic, then, delimits the theoretical field governing what is and is not to be included within it. It is, as he puts it, a "determinate unitary structure" unifying all the elements contained therein (*FM,* p. 67).

Althusser develops this concept of the problematic in the context of his controversy over the status of the works of the "young Marx" (*FM,* pp. 51-86). For some there is a direct continuity between Marx's early "ethical" writings especially, the *1844 Manuscripts* with their talk of "alienation" and the reappropriation of the "human essence" and the later or mature analysis of the production process in *Capital.* This continuity is said to be guaranteed by the reappearance in the later works of Marx of certain key concepts, notably "alienation," under the guise of the "fetishism of commodities." This is taken to prove that there is no break or caesura between the young and the old Marx as the guarantee of continuity is provided by Marx himself (Avineri, 1968, p. 40). Yet Althusser claims that this purported continuity between the young and old Marx is the result of a "naive" or "eclectic" reading which takes the literal presence of certain terms at their face value without pausing to consider whether or not the problematic that gives them their meaning had changed. The problem with this approach is that it introduces a sort of cryptoteleology into the study of ideas in its search for continuities and resemblances, germs and anticipations, of a later doctrine within an earlier one (*FM,* pp. 56-57). By doing this, a kind of false coherence or system is placed upon works that may in fact be radically different.

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[4] The origins of this theory can be found in Austin (1962) and Serle (1969); for a recent effort to develop this theory see Habermas (1979, pp. 1-68; 1984, pp. 288-295, 305-307, 319-328).

[5] For some critical commentaries see Tarcov (1982) and Shapiro (1982).

[6] See also Collingwood (1972, chap. 5).

[7] The critical literature on this subject is too voluminous to recount here; for a sample see Lakatos and Musgrave (1970); MacIntyre (1977); Bernstein (1978, pp. 84-93; 1983, pp. 51-93); see also Miller (1972, pp. 804-806).

One seemingly peculiar feature of the problematic upon which Althusser insists is that it plays the *active* role in the determination of a theory. Indeed, he attributes functions to the problematic that other epistemologies had done to the human subject. In words that we are enjoined to take "literally," Althusser claims that it is no longer the individual subject who thinks and constructs theories, but the problematic that thinks in and through the subject.

> The sighting is thus no longer the act of an individual subject, endowed with the faculty of "vision" which he exercises either attentively or distractedly; the sighting is the act of its structural conditions, it is the relation of immanent reflection between the field of the problematic and *its* objects and *its* problems. . . . It is literally no longer the eye (the mind's eye) of a subject which *sees* what exists in the field defined by a theoretical problematic: it is this field which *sees itself* in the objects or problems it defines—sighting being merely the necessary reflexion of the field on its objects. (*RC,* p. 25)

This attempt to dissolve the knower into the products of his knowledge is not altogether absurd. By drawing attention to the problematic as the unconscious infrastructure governing the "production" of particular utterances, Althusser wants to show that it is impossible for even the most careful writer to be the sole determiner of the text's meaning. Against the view that an author has some privileged access to his text's meaning, what is being suggested is that the writer's avowal of his own purpose is to some extent unnecessary once we have the text. What, for instance, Kant may have intended to prove by argument in the *Critique of Pure Reason* is one thing. What he succeeded in saying is another, which can be distinguished from his purpose in writing it. The upshot is that the finished text acquires an "autonomy" of its own apart from the author's private thoughts or mental states. As a critique of the unduly mentalistic concepts like will and intentionality, this is probably correct.[8] What is more difficult to discern is how this banishment of authorial or communicative intent affects the criteria for determining interpretive accuracy.

In the first place, the suggestion that a problematic or paradigm governs the thinking of a writer or speaker implies a sort of linguistic determinism which cannot account for varieties of linguistic usages. Although ordinary or conventional speech may well be determined by such closed and self-contained frames of reference, these cannot explain how a writer can and indeed often does challenge and go beyond existing linguistic boundaries and in the process say something new. This was certainly the case with Machiavelli, who endowed traditional moral concepts such as liberality, piety, and goodness with new meaning and implications. To attempt to explain Machiavelli's usages solely within the dominant problematic of his day would scarcely do justice to the novelty and originality of his thought. The same would also be true for the other great innovators in the history of political thought. Thus Althusser's linguistic determinism is at fault for viewing language solely in terms of the restraints and limitations that it imposes rather than the creative and dynamic propensities that it affords. Accordingly, when conceptual change is acknowledged, it can only appear as a radical break or rupture *(coupure)* with the past in which one self-enclosed problematic or framework of meaning takes the place of another. Such change is only possible because it breaks all the rules all at once, which is precisely what I want to argue *cannot* happen (*FM,* pp. 32-34; *RC,* pp. 27, 44-45).[9]

Second, Althusser is vague about how the problematic of a text can be identified and described. Rejecting the view that we can have access to a text through the communicative intent of the author, he seems to regard access not as something discovered by but as created or produced by the reader, the result of an ongoing dialectic between reader and text. But if the problematic is not something already there in the text, how do we know if we have in fact found it? Strictly speaking, we could not speak of finding it at all. The implication is that there can be no single correct reading of Marx or any author. There are only readings produced by the act of interpretation. But by denying that the recovery of an author's intention may serve as the foundation for interpretive accuracy, Althusser leaves himself open to the charge of rejecting any claims for scholarly objectivity or even serious debate among rival interpretations. Thus, to the question of why one should recommend or adopt Althusser's reading of Marx rather than the many others that have presented themselves over the last century, we could do nothing more than express a sort of resigned methodological agnosticism (Giddens, 1976, p. 63; Smith, 1984a, pp. 80-81).

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[8] An early attempt to exorcise the Cartesian "ghost in the machine" can be found in Ryle (1949, pp. 15-24).

[9] Althusser is indebted for his epistemological theories to Bachelard; for a sample of his writings see Bachelard (1934, 1938, 1968); an account of his epistemological history can be found in Lecourt (1969, 1975); see also Schmidt (1981, pp. 86-93).

To be sure, Althusser would not be happy with the relativistic implications of his views. After all, he has read and criticized the works of other interpreters—Engels, Plekhanov, Kautsky, Lukacs, Gramsci, and Sartre, to name just a few. Matters of interpretive accuracy cannot, therefore, merely be a matter of indifference to him. His manner of reading Marx assumes a position of superiority to those who have read him before. But in just what respect his reading is superior he is loath to say. Presumably to attempt to identify fixed criteria would be to fall back on the kind of "foundationalism" he has everywhere sought to reject. The most he is willing to concede is that our criteria of interpretive accuracy are internally bound up with the problematics that we are called upon to compare and evaluate. But far from answering this difficulty, this solution evades it. For if our criteria of validation already presuppose what it is that is to be evaluated, there is no way of determining whether one problematic, paradigm, or language game is to be adjudged in any way superior to or more adequate than any other. The result would be a circularity in which the problematics in question would be called upon to validate themselves.

In fact, as I shall show in the next section, this is precisely what Althusser intends in his rejection of the classical "problem of knowledge," the search for permanent foundations or guarantees for knowledge or truth. First, however, I must point out that the model of interpretation just enumerated prohibits us from engaging in a number of activities in which we characteristically do engage. Whenever we translate from one language to another, analyze critically the values and beliefs of societies different from our own, or even when Althusser criticizes other readings of Marx, we are implicitly assuming the existence of certain common criteria of evaluation that transcend any one problematic or form of life. Of course, that we characteristically do engage in these activities does not render them immune from criticism, but because we are able to rise above any one framework or form of life, we are able to depict either or both in terms of a more synoptic or comprehensive perspective. What I have in mind here is something like Gadamer's conception of a "fusion of horizons," where we are able to depict both our own and an alien way of life as alternative possibilities to certain human constants at work (Gadamer, 1975, pp. 269-274). One advantage of this approach is that it protects us against the claims of a presumptuous Cartesianism to transcend *all* problematics and forms of life without collapsing into ethnocentric prejudice or mere relativism. Thus, if Althusser is to salvage his own reading of Marx from epistemological relativism, it can only be by admitting to a great deal more overlap among different epistemic standards than his theory of problematics would be willing to concede.

## The Problem of Knowledge

"To conceive Marx's philosophy in its specificity . . . is to conceive knowledge as production" (*RC,* p. 58). In describing knowledge itself as a kind of production or practice, namely, "theoretical practice," Althusser has two aims in mind: one positive, the other critical. The positive goal is to highlight the "autonomous" character of theory. As opposed to other Marxist theoreticians who would explain all the basic forms of life activity as derivative from the economic sphere, Althusser proposes to consider the independence of theory (Deprun, 1970, pp. 67-82). "We must recognize," he says, "that there is no practice in general, but only distinctive practices" for "there can be no scientific conception of practice without a precise distinction between the distinct practices" (*RC,* p. 58). The autonomy of theory apart from the other practices that compose the "social formation" is guaranteed by its own "internalist" or "immanent" form of justification. For Althusser, knowledge is not verified either empirically by reference to "facts" or "brute data" or pragmatically as an expression of social needs or "interests" (as many Marxists might argue). Rather it is said to contain its own canons and protocols of validation that are internal to knowledge alone (*RC,* p. 141).

The claim that knowledge is "production" is also used as a polemical weapon against any form of epistemological "foundationalism." By foundationalism I mean here the search for transhistorical or transtheoretical measures, standards, or guarantees against which knowledge may be verified. For Althusser, the search for guarantees is a part of the traditional "problem of knowledge" which he rejects (*RC,* pp. 52-54). The problem with these previous epistemologies is the belief that absolute or a priori foundations could be discovered upon which knowledge or science could be grounded. An example of this foundationalism would be the claims embodied in classical empiricism. For the empiricist, everything that is to count as knowledge must be capable of verification through perceptual experience. This requirement is concretized in the famous "verification principle," according to which if our statements about the external world are to be meaningful, they must be testable against reality or some portion of it. Only knowledge based on experience can lay claim to objectivity. The problem here is simply that the principle of verifiability cannot itself be verified on perceptual or testable grounds. It therefore contradicts its own premises for determining what knowledge is, and this contradiction, Althusser holds, underlies all such attempts to ground knowledge. The same difficulty underlies the rationalist's search for such principles as self-evidence and logical inviolability as the basis for certainty. Because these principles must claim to precede the knowledge they are called upon to validate, they must either seek other justificatory principles by which to validate themselves or stand condemned of self-contradiction.[10]

Instead of attempting to determine a priori rules of evidence for what is to count as knowledge or how knowledge is to be verified, Althusser proposes a quite different procedure, where the knowledge or science in question is called upon to validate itself. The first step in this procedure is the argument that knowledge is not so much discovered as *created.* It is a form of "production" or "practice." He proposes a tolerably specific definition of what is meant by practice.

> By *practice* in general I shall mean any process of *transformation* of a determinate given raw material into a determinate *product,* a transformation effected by a determinate human labour, using determinate means (of "production"). In any practice thus conceived, the *determinant* moment (or element) is neither the raw material nor the product, but the practice in the narrow sense; the moment of the *labour of transformation* itself, which sets to work, in a specific structure, men, means, and a technical method of utilizing the means. (*FM,* pp. 166-67)

There are four distinct kinds of practice: economic, political, ideological, and theoretical (*FM,* pp. 167, 229). It is the "combination" or "articulation" of these practices that constitutes the overall "social formation" or "mode of production." Yet while each of these practices is distinct, they still share a common or "homologous" form: a raw material, "the labour of transformation itself," and a finished product. In the case of theoretical practice, the raw materials present themselves as a series of discrete mental events or ordinary, commonplace concepts. The point to be noted here is that evidence never presents itself as a world of "fact" or "brute data" open to direct inspection, but rather as an already existing universe of concepts or linguistic entities. It is not something immediately "given," but is a world already mediated by interpretation and judgment of an "ideological" sort. The raw materials of theoretical practice are, then, a type of thinking, but thinking at a very low level of intellection, what another tradition would call "prethetic" reflection or the standpoint of the "natural" attitude.

Characteristically, Althusser displays little concern with the diverse origins and natures of these commonplace concepts that form the basis for knowledge. Although they may precede the coming into being of science, they are an inert, pliant kind of stuff fashioned out of the exigencies of "lived experience." They are, for this reason, all but useless for scientific purposes, since science, as we have seen, comes into being only through a "rupture" with this experience. Accordingly, the decisive "moment" of any science consists in "elaborating its own scientific facts through a critique of the ideological 'facts' elaborated by an earlier ideological practice." To elaborate these "facts" is, moreover, "to elaborate its own 'theory' since a scientific fact—and not the self-styled pure phenomenon—can only be identified in the field of theoretical practice" (*FM,* p. 184). Whereas a theoretical practice may begin with an existing system of representations, its aim is to produce a "corpus of concepts" that both "rejects the old one even as it englobes it, that is, defines its 'relativity' and the (subordinate) limits of its validity" (*FM,* p. 185). The point is that theory recognizes no "facts" that do not already presuppose a prior theory. Even in science we move within a circle of interpretation that can never be brought to an end because it is always subject to further interpretation.

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[10] This critique is brilliantly developed by Hegel (1971, pp. 131-145); for an excellent commentary see Norman (1976, pp. 9-28).

The direction, then, of Althusser's "knowledge = production" thesis is away from any search for epistemological foundations or transcendental guarantees for knowledge and toward some form of internal or immanent criteria of validation. By an internal or immanent (as opposed to a foundationalist) theory of knowledge I mean one that seeks validity claims *within* the knowledge to be verified. In contrast to the claims of a Kantian "first philosophy," which seeks to establish once and for all the conditions of possible knowledge, Althusser maintains that standards of cognitive acceptability are already at hand within existing sciences or "theoretical practices." There is no point, he believes, in trying to establish ideal rules to see whether our knowledge measures up or "corresponds" to an independently existing external world, but rather to see whether the knowledge in question lives up to its own self-imposed standards of adequacy. This, he hopes, will put to rest once and for all the traditional problem of knowledge.

> *Theoretical practice* is indeed its own criterion and contains in itself definite protocols with which to *validate* the quality of its product . . . No mathematician in the world waits until physics has *verified* a theorem to declare it proved, although whole areas of mathematics are applied in physics: the truth of his theorem is a hundred per cent provided by criteria purely *internal* to the practice of mathematical proof, hence by the *criterion of mathematical practice,* i.e., by the forms required by existing mathematical scientificity. (*RC,* p. 59)

By insisting that theory supplies its own internal criteria of acceptability, Althusser is enabled to avoid the pitfalls of the traditional theory of knowledge. More positively, by adopting the standpoint of an internalist theory, he not only provides an alternative to foundationalist epistemologies with their search for transcendental truths or guarantees but is in accord with much of the recent work done in this area. In the later works of Wittgenstein, for instance, it is argued that epistemic standards must be sought not in some sort of "metalanguage" or "first philosophy" but within established ways of proceeding.[11] The use of language itself provides us with criteria of reflective acceptability, so that it is only necessary to check our judgments against existing linguistic standards to discover whether or not we are justified in making them. Nevertheless, the procedure of immanent validation raises serious difficulties of its own, namely, an uncritical relativism concerning the content of knowledge. Since Althusser's internal criteria are strictly relative to existing ideological or theoretical practices, they can tell us nothing about the truth of these practices.

In the first place, it could be argued that although the logicomathematical procedures of proof referred to in the above passage may be valid for certain restricted areas of scientific inquiry, they cannot hold true for the social sciences, which are bound by the demands of fact or evidence. If there is no body of hard factual evidence that is taken to be objective and independent of the inquirer into it, then we have no compelling basis on which to distinguish truth from falsity. The result of Althusser's rejection of the "problem of knowledge" is to render theory immune to empirical falsification (Callinicos, 1976, pp. 59-60; Thompson, 1978, pp. 10-13, 16-25, 33-35, 39-40). Althusser admits as much when he says that it is necessary to "purify our concept of the theory of history . . . of any contamination by the obviousness of empirical history" (*RC,* p. 105). But the question remains, if every theory has its own immanent criteria of validity, how is it possible to compare and evaluate different theories? On this account it would seem to be impossible to choose rationally between, say, a scientific and a theological account of the origin and development of the species or between a Marxist and a liberal conception of the state, since both could be held to contain their own immanent logic and standards of intelligibility. The point is that unless our conceptions are at some level based on fact, our whole construction could be rigorously coherent and still be a delusion.

To some extent Althusser's relativism stems from the philosophical source from which his epistemology is culled. The doctrine that "truth is its own criterion" is taken directly from Spinoza, whom Althusser regards as "the only direct ancestor of Marx" (*RC,* p. 102; cf. *ESC,* pp. 132-41).[12] In the *Ethics* Spinoza had used this doctrine to indicate the *systematic* character of truth, that truth lies in the whole (Spinoza, 1952, p. 115).[13] To describe a statement or proposition as true for Spinoza is not to say that it adequately pictures or represents the world. Rather truth lies in the relation between one statement and some logically "concatenated" set of statements that it presupposes. Spinoza could, however, adopt this rigorously coherentist theory of truth and still avoid epistemic relativism because he also enlisted the support of a monistic metaphysics to guarantee that the order and connection of things always remains parallel with the order and connection of ideas. *"Ordo et connexio idearum rerum idem est, ac ordo connexio rerum"* (Spinoza, 1951, p. 86). Lacking any such "psycho-physical parallelism,"[14] Althusser can offer no such promise that the real object *(objet réel)* will in any way coincide with its conceptual representation *(objet de pensée)* (*RC*, pp. 35-42, 62-63, 66-67).

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[11] Wittgenstein's turn from ontological realism toward conventionalism has been noted by Rosen (1969, pp. 1-27).

[12] For the relation between Althusser and Spinoza see Eco (1968, p. 360); Deprun (1970, pp. 77-79); Anderson (1976, pp. 64-66).

[13] Spinoza's theory of truth is treated in Hampshire (1962, pp. 86-90, 97-107).

Althusser's response here is to dismiss the whole problem of knowledge, that is, the verification and empirical validation of truth claims as "ideological." In his own words: "We can say, then, that the mechanism of production of the knowledge effect lies in the mechanism which underlies the action of the forms of order in the scientific discourse of the proof" (*RC*, p. 67). But this offhand dismissal of the problem of foundations by no means succeeds in resolving our difficulties. Without some rules of procedure independent of the practice in question, not only will we be unable to decide between competing accounts but we will be thrown back into the morass of epistemic relativism discussed in the last section.

This raises a further problem. Not only does Althusser's rejection of foundationalism eliminate the constraints of evidence, it tells us nothing of how criteria themselves change over time. Curiously, Althusser's argument that every problematic contains its own internal norms of intelligibility is not unlike the position argued by Winch (1958). In discussing the differences between the claims of theology and those of science, Winch suggests that "intelligibility takes many and varied forms" and that there is no "norm for intelligibility as such." "For instance," he writes, "science is one such mode and religion is another; and each has criteria of intelligibility peculiar to itself. So within science or religion actions can be logical or illogical. . . . But we cannot sensibly say that either the practice of science itself or that of religion is either illogical or logical; both are non-logical" (Winch, 1958, pp. 100-101).

In other words, just as Althusser maintains that no mathematician requires the aid of physics to verify mathematical theorems, so Winch is suggestings that both science and theology have their own internal criteria of intelligibility peculiar to themselves and as such must be judged simply on their own terms. These criteria can tell us what is to count as true or false, "logical or illogical" within the disciplies or modes of life in question. But they cannot tell us how these disciplines or modes of life can be compared or evaluated, or whether or not they make sense. There is a kind of latent positivism within this approach according to which various theories and language games merely *are* and, as such, must be accepted. There is a paradoxical agreement between Althusser and Wittgenstein's dictum that "What has to be accepted, the given, is—so one could say—*forms of life*" (Wittgenstein, 1968, p. 226). The result of what Gellner (1974, p. 20) has called this "skeptical abstinence from transcendent claims" is to re-endorse uncritically those patterns of life and interaction that merely happen to exist. And this is precisely what we need not, and ought not, to accept.

But this seeming acceptance of the inviolability of our theoretical practices glosses over the discrepancies that can arise not only between but within forms of life, language games, and problematics. The criteria of reflective acceptability operative at any one time are not monolithic; they may not only contain internal incoherences and contradictions but may be challenged by rival practitioners of the same field. Thus Alasdair MacIntyre has said in response to Winch that "at any given date in any given society the criteria in current use by religious believers and scientists will differ from what they are at other times and places. *Criteria have a history*" (MacIntyre, 1979, pp. 66-67; emphasis added). But if the criteria change, they may not be considered all of one piece, and to refer to them as though they were misses a crucial dimension of epistemological change. The more important point is why at some points in the history of thought existing criteria for validating knowledge claims are regarded as satisfactory, whereas at other times anomolies and incoherences become so great as to sanction as "epistemological break."

Finally, Althusser's insistence on the "radical inwardness" of our criteria leads to a severence of theory from practice. He begs the question by referring to theory itself as a kind of practice, but such a conception is totally at odds with Marx's more pragmatic theory of truth (Kolakowski, 1968, pp. 38-66). The basic point of departure for all of Marx's epistemological reflections is the conviction that the relation between the species and its environment is fundamentally one of *need.* As Marx indicates in a number of places, reality itself is a human creation, and theory, man's "practical consciousness," our awareness of the world of things, is defined by its ability to assist us in appropriating the world as the sum total of the possible objects of need. Thus, in the *German Ideology* we find statements like "consciousness is . . . from the very beginning a social product and remains so as long as men exist at all," and more specifically that "language is as old as consciousness, language is practical consciousness that . . . only arises from the need, the necessity, of intercourse with other men" (Marx, 1970a, pp. 49-50).

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[14] The emphasized term comes from Collingwood (1942, p. 8).

The implication here is that all thinking is inherently practical or problemsolving in nature. Not truth but success in practice, success in enabling us to acquire the objects of need, is the primary category of evaluation. As Marx indicates in his second *Thesis on Feuerbach*: "The question whether objective truth can be attributed to human thinking is not a question of theory but is a practical question. . . . The dispute over the reality or non-reality of thinking which is isolated from practice is a purely scholastic question" (Marx, 1970a, p. 615). The conception, then, of a "theoretical practice" unconnected to the satisfaction of practical needs would have struck Marx as a typically idealist fantasy.[15]

There may be good intellectual reasons for rejecting Marx's pragmatist or instrumentalist conception of knowledge. After all, not all thinking (e.g., the playful examination of ideas in a Platonic dialogue) is ruled directly by practical necessity. Yet by cutting theory off from any foundation in either nature or social practice, Althusser's solution to the "problem of knowledge" remains subject to the same strictures Marx levelled against Hegel and the German idealists of the 1840s. The idealism of Althusser consists not in the denial of an independently existing external world, but in the positing of a self-generating conceptual universe with no ties to social practice and claiming its own immanent criteria of validity. There is, in the final instance, no interplay or dialectic between "social being" and "social consciousness," but, as with all systems of idealism, the latter is given an "autonomy" of its own apart from the sordid world of politics and history.

Althusser's answer, then, to the traditional "problem of knowledge" is the idea of an immanent critique. An immanent critique is one that rejects the search for natural or apodictic starting points for knowledge and the picture or correspondence theory of truth that accompanies it. Knowledge, one could say, both begins and ends within the sealed chamber of "theoretical practice." "It is perfectly legitimate to say," Althusser remarks, "that the production of knowledge which is peculiar to theoretical practice constitutes a process that takes place *entirely in thought*" (*RC*, p. 42). The idea that there are neither transcendent standards, as the Platonist believes, nor "brute data," as the positivist argues, against which our knowledge can be judged adequate testifies to a condition that Rorty (1982, p. xxxix) has described as "decadence" or what I prefer to call left-wing Kantianism.

Althusser's position is "Kantian" because it claims that the mind has no direct access to reality independent of or apart from the linguistic practices that we use to describe, explain, and evaluate it. It is "left" because it maintains that even our most deeply held convictions and beliefs have no permanent fixity or foundation but are the products of a critique of earlier theoretical and ideological practices which are themselves liable to epistemological upheaval in the future. As we shall now see, Althusser's own theoretical practice is premised on a critique of the humanistic conception of the self as a knowing and acting subject. This essentially Cartesian understanding of the subject as possessed of such attributes of freedom, dignity, and rights is not only said to be based on a philosophically false anthropology but one that may now be nearing its end.

## The Death of the Subject

Althusser's attack upon the Cartesian search for foundations of knowledge and action in the thinking ego has its final end or purpose in the rejection of any philosophical anthropology based upon the conception of "man." In his own words, Marxism must become a "theoretical antihumanism" (*FM*, pp. 229-231, 241). By antihumanism Althusser means that the self, the human subject, does not so much constitute but is constituted by the structural, systemic relations in which it finds itself. It is the belief not that men make history but that history makes men or that history makes itself that defines this movement (*ESC*, pp. 35-57). As I shall show later, this antihumanism forms the Althusserian pendant to the structuralist dictum that "man is dead" (Dallmayr, 1981, pp. 21-29; Smith, 1984a, pp. 192-200).

This theoretical antihumanism has as its purpose the decentering of the subject. Indeed, this act of decentering has been crucial to every modern discipline claiming scientific status. As Freud has indicated, the "naive self-love" of man has become progressively decentered by the advances of modern science (Freud, 1969, pp. 284-285). The first blow fell when Copernicus discovered that the earth was not the center of the universe but a tiny speck within a cosmic space of infinite vastness. The second blow came when

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[15] Althusser has made some efforts to correct this "theoreticism" (i.e., idealism); see *FM*, p. 15; *ESC*, pp. 119-125; for evaluations of these "self-criticisms" see Callinicos (1976, pp. 107-114) and McCarney (1980, pp. 70-79).

Darwin dislodged the species from its supposedly privileged position within creation, showing it to be just one in a long line of evolutionary forms. The third blow fell when Freud himself showed that the ego, once regarded as the sovereign subject, is not even "master in its own house" but is directed by deeper unconscious drives and purposes. And finally, this process of decentering has culminated in our own day when Althusser, Foucault, and Lévi-Strauss have tried to eliminate the influence of man altogether as a subject of historical and ethnographic research.[16]

Althusser's own attempt to decenter the subject has taken the form of an attack on humanism and Enlightenment philosophies of "man." The distinctive feature of traditional humanism, that of Feuerbach for instance, has been the desire to recover or reappropriate for man those attributes that traditional philosophies had ascribed to God or some metaphysical Absolute (*ESC,* pp. 195-207). The most important of these attributes is the capacity for free choice or creative causality. Indeed the belief that we are free agents capable of initiating and therefore responsible for our own actions stems originally from Christianity and the "myth" that God addresses man "by name" (*LP,* p. 166). Humanism is, then, merely an inverted theology. It merely substitutes a generic mankind or human subject as the omnipotent knower and maker of the world. To be sure, this dependence of humanism on theology may be a negative one, but it is a dependence all the same.

The Althusserian critique of humanism itself follows a line of thought first developed by Heidegger in the *Letter on Humanism* (1977, pp. 193-242). There he argued that the "atheist" existentialism of Sartre was nothing more than an outgrowth of the Western "metaphysical" tradition. This metaphysical tradition, stretching from Plato to Marx, has consisted of a series of increasingly disastrous attempts to impose our own conscious designs and efforts on nature. Metaphysics conceals a fundamentally technical interest in gaining mastery and control over both the natural and the social worlds. Such anthropocentric humanism has led to the subjugation and exploitation of all beings, including man, by an aggressively self-centered humanity. Humanism is thus a form of anthropocentric hubris: the deification of man instead of God. To eliminate any such metaphysics of the subject, it is necessary to decenter or dissolve man by returning to a more primordial sense of Being. Only when the human is understood as rooted in rather than standing over Being can the arrogance of humanism be held in check.

Following Heidegger, Althusser has repudiated humanism not as a "metaphysics" but as an "ideology." Ideology is to be understood here in contrast to science. Ideology, Althusser writes, is distinguished from science in that in ideology, "the practico-social function" dominates the theoretical function (*FM,* p. 231). Elsewhere he comments that ideology is governed by certain "interests" beyond the requirements of knowledge alone (*RC,* p. 141). Ideologies perform the further task of "interpellating" individuals as "subjects." The category of the subject, like that of ego or consciousness, is said to be "constitutive of all ideology" or "all ideology has the function (which defines it) of 'constituting' concrete individuals as subjects" (*LP,* p. 160). It follows, therefore, that ideologies are ensembles of false beliefs not, as Marx believed, because they interpret the world from the standpoint of any one particular class within society, but because they are tied to the claims of a "constitutive subject" or of individuals as self-directing agents. The conception, then, of "a subject endowed with a consciousness" must be rejected as an "absolutely ideological 'conceptual' device" (*LP,* p. 157). It follows further that Marxism qua science must be subjectless, that is, a "process without a subject," a process without end or purpose, unilluminated by any rational necessity or transcendent Absolute (*PH,* pp. 181-183; *ESC,* pp. 94-99). Once history, conceived as a rational or meaningful whole, can be shown to be unintelligible or opaque in its essence, it becomes possible to reduce "the philosophical (theoretical) myth of man . . . to ashes" (*FM,* p. 229).

Althusser's own form of antihumanism begins with the denial that man or even groups of men form the primary unit of Marx's analysis. Marx, Althusser tells us, was not interested in man as such but in certain "ever-pregiven" relations of production which distribute the roles and functions that agents play out in their daily lives. Rejecting Marx's early philosophical anthropology as tainted with the "ideology" of man and its belief in a substantive human nature ("species being"), Althusser cites approvingly from Marx's last work, his *Marginal Notes on Adolph Wagner's "Handbook of Political Economy"* to show that by the end of his life Marx had left the last vestiges of his earlier humanism far behind.[17]

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[16] See Nair (1974, p. 169): "With Sartre there is too much history, with Lévi-Strauss, Foucault, Althusser, and Lacan there is no longer any history; yesterday object and structure were dissolved into the subject; today subject and consciousness are buried in the object. The soul of the world was free and conscious choice, now the unconscious is king and the world has lost its soul."

[17] Poster (1975, pp. 67-71); Schaff (1978, pp. 128-139); Kolakowski (1978, p. 486).

"My analytical method," Marx says there, "does not start from man but from the economically given social period" (Cited in *FM*, p. 219). Not our lived experience but the various structures of social life—economic political, ideological—constitute the real "subjects" of history.

WIthin Marx's own writings, Althusser finds warrant for this antihumanism in Marx's use of the term *Träger*, which is a fairly commonplace German word that means literally "bearer" or "support." Althusser does not, therefore, merely import this term from the outside to give his Marxism a structuralist turn. Rather it is a concept that Marx uses regularly throughout *Capital* to show the way in which economic agents are turned by capitalism into "personifications" of the social relations of production. Thus at one point we read that, "In the course of our investigations we shall find, in general, that the characters who appear on the economic stage are but the personifications [*Träger*] of the economic relations that exist between them" (Marx, 1970b, p. 85). The difference is that whereas Marx uses this concept as a means to indict capitalist society for treating men as things, Althusser uses it to show that the very idea of man is a chimera that we would be better to drop altogether.

Althusser uses Marx's *Träger* concept to show that economic agents are never anything more than "bearers" or "functionaries" within a given mode of production. There are only a limited number of places and functions that these agents can occupy which are continually reproduced and continually develop what they demand of their occupants. Men cannot, therefore, be regarded as active creators or makers of this process. They are never anything more than its "supports."

> The structure of the relations of production determines the *places* and *functions* occupied and adopted by the agents of production, who are never anything more than the occupants of these places, insofar as they are the "supports" [*Träger*] of these functions. The true "subjects" (in the sense of constitutive subjects of the process) are therefore not these occupants or functionaries, are not, despite all appearance, the "obviousness" of the "given" of naive anthropology, "concrete individuals," "real men"—but *the definition and distribution of these places and functions. The true "subjects" are these definers and distributors: the relations of production* (and political and ideological social relations). (*RC*, p. 180)

Elsewhere Althusser goes so far as to suggest that the production and reproduction of social life is not something carried out by intelligent agents with awareness of what it is they are doing, but takes place, so to speak, behind the backs of men who are never anything more than the supports of the "mode of production" within which they find themselves. There is no action in the Althusserian universe where this is understood as purposive behavior directed toward the pursuit of some freely chosen end or purpose. There is only a set of reactive responses determined "in the last instance" by the needs of the production process. The stage, Althusser says, is set and the players merely perform according to scripts already written out for them in advance.

> Now we can recall that highly symptomatic term "*Darstellung,*" compare it with this "machinery" and take it literally, as the very existence of this machinery in its effects: the mode of the stage direction (*mise en scène*) of the theatre which is simultaneously its own stage, its own script, its own actors, the theatre whose spectators can on occasion, be spectators only because they are first of all forced to be its actors, caught by the constraints of a script and parts whose authors they cannot be, since it is in essence *an authorless theatre.* (*RC*, p. 193)

These passages provide literally textbook examples of what Dennis Wrong (in another context) has called the "oversocialized conception of man" (Wrong, 1961, pp. 183-193; cf. Zetterbaum, 1971, pp. 240-246). On this account what we are is so completely determined by the "places and functions" that we occupy that there is nothing else left over. There is no Archimedian point of exemption of the kind searched for by Descartes, no transcendental subject of the Kantian or Husserlian variety capable of standing outside experience, for the fact is that we are conditioned beings all the way down. By rejecting the idea that there is a permanent nature or essence of man which is, to be sure, shaped and molded by culture but which may also stand in a position of conflict or tension with that culture, Althusser has provided a structuralist conception of the self as a personification or passive functionary of the empirically determined circumstances in which it finds itself. The oversocialized conception of the self aims to provide an answer to the age-old question, "How is society possible?" or "What are the sources of social order?" by claiming to demonstrate that we are exclusively the products of the social roles we inhabit.

The idea, of course, that human beings are molded by circumstance or that we become what we are in and through interaction with others has long ceased to be controversial. One may be a feudal lord, a serf, a property-owning bourgeois, or a university professor, and to an extent one's behavior is determined by the norms and expectations governing those roles. But to say that *everything* we do (or refrain from doing) can be explained by the "places and functions" we occupy is by no means obvious. Althusser is so concerned to deny the humanist thesis that men make their own history and in doing so make themselves that he overlooks the fact that the production and reproduction of social life are above all skilled performances sustained and made to happen by intelligent social agents who act in the light of their understood situations. Seen in this way, individuals are never merely representatives of a set of prescribed social roles, nor can human actions be explained solely in causal or functional terms, that is, in terms of how they keep the system going. Social actors must be understood at least in part as intentional subjects acting in response to an understood situation and whose actions must also be seen in terms of their symbolic or meaningful character for the agents themselves. It is the meaningful side of human action that Althusserian structuralism fails to grasp (Giddens, 1976, pp. 44-48, 74-77; Winch, 1958, pp. 43-53, 116-120).

It should now be clear that the aim of this "oversocialized" conception of the self is to jettison whatever permanent core or substance was once thought to belong to a human being and to substitute in its place the conception of the self as a bearer or functionary, a "role player" as we might say.[18] When, for instance, premodern thinkers regarded the self, they thought largely in terms of character or even soul. The soul in turn was thought to have a distinct nature defined by a hierarchy of needs dictated by the concerns for human excellence and nobility. When even modern thinkers like Kant spoke of the essential freedom or "dignity" of man, he meant not each individual but the totality of the species as "represented" by or embodied in the individual subject. In contrast, then, to this search for any defining center or core to which the universal concept of man might refer, Althusser prefers to dissolve the self into the social *relationships* which uniquely determine it. Indeed, he is fond of quoting from Marx's sixth *Thesis on Feuerbach* as an anticipation of this decentered self. "The human essence," Marx says there, "is not an abstraction inherent in each individual . . . it is the ensemble of social relations" (Cited in *FM,* pp. 227-228, 242-243).

This dissolution of the self into its nexus of social relations is no mere eccentricity on the part of Althusser. His denial of any finality or fixity is, as I have already indicated, part of a broader counter movement now underway in contemporary social and political theory whose outcome has been declared under the slogan "the death of man." Along with Althusser, the most prominent member of this school has been Michel Foucault, who has written that Nietzsche's declaration of the death of God has been followed one hundred years later by the end of his murderer. In fact the conception of man as an agent or center of initiative endowed with the capacity for free, purposive, and responsible action is only as old as Rousseau and Kant. "Before the end of the eighteenth century," Foucault writes, "man did not exist . . . he is a recent creature which the demiurge of knowledge fabricated with its own hands less than two hundred years ago" (Foucault, 1970, p. 308). But just as this "creature" has come into being, so too in our own day do we see it beginning to pass away. "As the archaeology of our thought easily shows, man is an invention of recent date. And one perhaps nearing its end" (Foucault, 1970, p. 387).

This proclamation of the end or death of man should not be taken simply as a prophecy of doom. The dissolution of the subject does not mean that human beings as a species are bound to disappear. What will disappear is not man as such but one historically specific conception of man as a thinking and active subject who is simultaneously both knower and maker of the world. What will also disappear is the attempt to ground the human in terms of purportedly permanent attributes like freedom, autonomy, dignity, or rights. These categories and concepts belong to only one epoch in the "archaeology of knowledge," which is now nearing its end. Thus it is the search for some more basic and permanent foundation to the self that is destined to disappear, "like a face drawn in the sand at the edge of the sea" (Foucault, 1970, p. 385).[19]

This dissolution of the self with which I have been concerned here results from a critique of Enlightenment humanism that Althusser shares with some of the most powerful thinkers of our age. By contrast to the conception, extending as far back as Protagoras, that "man is the measure of all things," the Althusserian critique of humanism stresses the insurmountable limitations imposed upon man by those non-human or extrahuman relations of production, the "places and functions" that we occupy. Man is no longer conceived as the active creator of the world, but as a supporting agent in a complex web of relationships whose axis revolves around the relations of production.

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[18] This is developed by Goffman (1959); for an excellent critique of this kind of sociologism see Wrong (1961, pp. 183-193); see also Smith, (1984b, especially pp. 528-535).

[19] A more developed account than I am able to give here can be found in Garaudy (1969, pp. 229, 238, 241-250); Poster (1975, pp. 334-339); Dreyfus and Rabinow (1982, pp. 28-43).

Like Spinoza's Substance or Heidegger's *Dasein,* these "structures" denote the preexisting world that is the product of neither individual or collective design and of which human agents are ultimately attributes. The result of this critique has been an astonishing reversal of the teaching of Descartes. Rather than "the masters and possessors of nature," we have become its servants.

## Conclusion

The Althusserian critique of humanism and its search for substantive foundations for both knowledge and action is directed against some of the most deeply held convictions of the western liberal tradition. The most cherished of these convictions is man, the human subject, whose moral worth resides in a capacity for reasoned judgment and the ability to conform conduct to the dictates of this judgment. The capacity for judgment, and of free agency that this implies, is the distinctive mark of the human. What distinguishes liberalism not only from classical political philosophy but from the variety of modern antiliberalisms is its uncompromising insistence on the power of autonomous human reason as the basis of our claim to be treated with equal "concern and respect."

The attempt to displace this predominantly liberal conception of man as an active and knowing subject, the bearer of rights and obligations, is at bottom what Althusser understands by "Leninism" (*RC,* p. 141). Leninism, the justification of rule by an elite or a central committee, is only possible when human beings are denied the capacity to act, the ability to make or create their history, and are instead regarded as an inert or recalcitrant mass, there to be molded and manipulated by a class of master technicians or engineers. Indeed, Althusser's seemingly benign insistence on the "autonomy" of theory is but one more sophisticated way of justifying the dominance of the Marxist intelligentsia claiming to speak for or in the true interests of the proletariat. The result has been the creation of an inward-looking, self-sufficient theoretical culture with no generic ties to the working classes, a development, needless to say, that Marx scarcely intended or expected to happen (Berki, 1975, p. 72). This goes some way toward explaining, I think, why this kind of Marxism has met with widespread acceptance in the universities and among the intellectuals while being ignored by ordinary people.

There is, of course, a final irony in this situation. If Marx was correct when he argued that the ideas of thinker must in some sense express the values or interests of his class or social position, then this must be *ipso facto* true for Althusser and his contemporaries. Althusser, I would suggest, is part of a new technocratic intelligentsia which is now in the process of formation. That such a "new class" of Marxist intellectuals is now seeking to provide itself with some legitimacy is a phenomenon attested to by writers on both sides of what it is no longer fashionable to call the Iron Curtain (Djilas, 1957; Kristol, 1976; Parkin, 1979). It is a distinct possibility that a theory that regards men as attributes of agentless structures may well express a reality dominated by impersonal bureaucratic, if not to say technocratic, methods of control. Such methods may well reduce human agents to *Träger,* mere functionaries rewarded by their ability to adapt or respond to the exigencies of the system of production. It is an arresting thought that Althusser's Marxism without a knowing subject is the best expression to date of the soul of modern bureaucratic rule, what one critic has called "the rule of nobody" (Arendt, 1969, p. 81).

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# Alexis de Tocqueville on Political Science, Political Culture, and the Role of the Intellectual

JAMES CEASER
*University of Virginia*

*According to Tocqueville, the most important determinant of the character of any society is its political culture* (moeurs). *A political culture is shaped not only by sociological conditions and laws, but also, in modern times, by ideas propounded by intellectuals. In Tocqueville's day, two dominant schools of thought were contending for influence over the public mind in Europe: philosophe rationalism and traditionalism. Neither one of these schools, Tocqueville argued, promoted a political culture that could reconcile liberty and democracy. Tocqueville conceived his "new political science" as an alternative to these schools that could meet this challenge. Unlike the opposing schools, the new political science could not be propagated directly as an ideology. Its implementation relied on an indirect strategy—using institutions to inculcate certain "mental habits" among the citizens. This in turn called for ways of limiting the role of intellectuals in influencing political culture.*

Alexis de Tocqueville left America in 1832 with the intention not merely of describing its political and social institutions, but of creating a "new political science for a world itself quite new" (p. 17).[1] The object of the new political science was to clarify the forces shaping the modern world and to provide "those who now direct society" with the knowledge to meet its greatest challenge: reconciling liberty and democracy (the social state characterized by an "equality of conditions") (pp. 9, 12). As Tocqueville told John Stuart Mill in 1836, "America was only the frame, my picture was Democracy" (*Mem 2,* p. 38). Mill recognized the scope and novelty of Tocqueville's project when he described *Democracy in America* as "the first philosophical book ever written on Democracy as it manifests itself in modern society" (1961, p. 122).

Tocqueville addressed *Democracy in America* to the entire generation of modern readers, but his immediate audience was clearly European, not American. No narrow chauvinism was at work here, but instead a frank assessment of the status of philosophy in different nations of the world. Nineteenth-century America hardly needed and could scarcely digest a work like *Democracy in America:* "The spirit of the Americans is averse to general ideas. . . . They pay very little attention to the rival European schools [of philosophy]. Indeed, they hardly know their names" (pp. 301, 429). America at the time lay at the intellectual periphery, but for the moment at least ignorance was bliss. Without either a taste for abstract political thinking or a large class of intellectuals, America alone had succeeded in reconciling democracy and liberty. Nor was this a mere coincidence: America, Tocqueville contended, had met the challenge of the modern age not in spite of, but in large measure because of, the lack of influence of recent doctrines in political philosophy and the absence of a class of intellectuals to spread them.

According to Tocqueville, the role of political philosophy in Europe could hardly have been more different. Contemporary thought, purveyed by a powerful intellectual elite, had a tremendous influence on European societies. Political philosophy in Tocqueville's time was divided between two basic schools that were openly competing for dominance over the public mind—philosophe rationalism (the more influential school) and traditionalism. In Tocqueville's view, the tenets of both of these schools tended ultimately to undermine liberty. Political thought had lost a constructive role as a guide to political action and was promoting the march towards despotism. This intellectual crisis demanded a theoretical response—a new political science—the rudiments of which Tocqueville discovered in his study of America's ostensibly unsophisticated political culture. Seldom has an anthropological expedition yielded so much for a philosophic debate.

Although not the first to study what political scientists today call political culture, Tocqueville applied that concept to the analysis of democratic states with greater rigor than any previous thinker. The character of a regime, in Tocqueville's view, is determined more than anything else by its *moeurs,* defined as "the sum of the moral

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[1] *Democracy in America* is cited by page number without any title abbreviation. Tocqueville's other texts are cited by the abbreviations noted in the reference list.

and intellectual dispositions of men in society . . . the habits of heart . . . [and] the sum of ideas that shape mental habits" (pp. 305, 287). The importance Tocqueville ascribed to *moeurs* (or political culture) accounts both for the scope of his political science, which extends to what we today would consider to be sociological and cultural elements of society, and for its distinctive method of studying a political system, under which institutions are analyzed as much in terms of their impact on the mental habits of the citizens as by their internal efficiency. Of all the aspects of the American regime, it was the *moeurs* of the people that Tocqueville believed best accounted for America's success. The United States was characterized by less brilliance and possessed less efficient political institutions than some nations in Europe, but it had developed the mental habits that could sustain liberty (pp. 307, 308).

In searching for the factors that form a political culture, Tocqueville, following Montesquieu, discussed the influence of such fundamental causes as geography, history, and laws.[2] Because these factors are in some degree particular to the experience of each nation, it is more correct in discussing Tocqueville's thought to speak in the plural of political cultures than of a single political culture. Yet what struck Tocqueville about the modern age was the emergence of certain factors that might influence all developed nations along the same lines: "Democratic peoples . . . in the end come to be alike in almost all matters. . . . [T]he same ways of behaving, thinking and feeling are found in every corner of the world" (pp. 660, 615). The two most important forces Tocqueville identified as causes of this common development were the spreading social condition of equality and the emergence of modern political thought as an active force capable of influencing the political cultures of all societies.

To understand the possibilities implicit in this path of development, Tocqueville abstracted from the particular nations and political cultures of his day (e.g., the United States, France, and England) in an effort to discover the picture of a common regime or political culture (pp. 417-429). This abstract regime, which Tocqueville originally referred to as "Democracy," consisted of a set of properties and tendencies that, if left to their normal course of development, would more likely eventuate in despotisms, whether of the usual or new tutelary variant, than republics. Although these tendencies operated in some degree on all modern societies, they were refracted by the special characteristics of each nation's political culture, with the result that different nations were more or less likely to control the despotic tendencies of democracy and establish liberty. Tocqueville chose the United States as the obvious place to study the properties of democracy, but it turned out that the United States was in many respects atypical of his general model of democracy, mainly because of the special intellectual foundation of its political culture.[3]

This article proposes to demonstrate that Tocqueville intended his new political science as an alternative to rationalism and traditionalism that could help shape the political culture of modern states to avert despotism. In light of the threat to liberty posed by these rival schools, Tocqueville held that political science could not remain aloof or detached, but had to become an active force in history. Yet in no sense did he conceive of the new political science as an ideology that could convince public opinion by the force of its popular appeal. Rather, it represented a complex teaching that synthesized certain epistemological elements of rationalism and traditionalism into a truer picture of reality that was intended to help his contemporaries make responsible choices about their future. The factors shaping the development of democracy, although increasingly common to all nations, were not entirely beyond human control, either within each nation or within the developed world as a whole. The decisive choice between freedom and despotism would be largely determined by the doctrines and ideas that guided people's thought (p. 705).

In developing this thesis, it will be helpful at the outset to indicate the different parts of the argument. I begin by tracing Tocqueville's account of the development of rationalism and traditionalism, turn next to a sketch of the purpose and general character of his new political science, then outline his strategem for inserting the teachings of the new political science into society, and finally indicate the manner in which he mixed rationalism and traditionalism to create his distinctive synthesis.

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[2] As Tocqueville wrote in a letter to Kergorlay in 1836, "There are three men with whom I commune a little every day; they are Pascal, Montesquieu, and Rousseau." Of these Montesquieu seems to have been the most influential, and 15 years later Tocqueville wrote the same friend that he was modeling his next work, *The Old Regime,* on Montesquieu's *Considerations on the Causes of the Grandeur and Decadence of the Romans* (Herr, 1962, pp. 48-49).

[3] Tocqueville had trouble with his use of the term democracy to describe a regime that was not free, and by the end of his life ceased using the term in the earlier sense. See his notes for the second volume of *The Old Regime,* the key passage of which is translated in Aron (1968, pp. 240-241).

## Rationalism and Traditionalism

Tocqueville's point of departure in both of his major works was the reaction of his contemporaries to democracy and to the shattering event that brought democracy to the center stage of European consciousness, the French Revolution. On one side, according to Tocqueville, stood the proponents of a radical version of rationalism, who saw themselves as the heirs of the French philosophes. For these thinkers, the French Revolution marked the dawn of a new era in which philosophy for the first time became an active force in transforming society. Philosophy stripped away the cloak of tradition that had supported superstition, injustice, and parochialism, providing in its place universally valid principles, derived from reason, that could instruct the enlightened on how to re-make society and construct new political orders (pp. 429-433; *OR*, pp. 13-169).

On the other side were the proponents of traditionalism or "organicism," who turned for their inspiration chiefly to the writings of Edmund Burke. This school, which emerged as a conscious reaction to philosophe ideas, attacked the abstract, "speculatist" character of rationalist thought, seeing in its universalism and contempt for tradition a dangerous doctrine that threatened political stability, diversity, and the noblest elements of civilization. Only by a renunciation of this doctrine and a return to respect for tradition—meaning an appreciation for hierarchy, diversity, and transcendence—could the world be saved from the ambition of the philosophes, who, according to Burke, had ushered in "a barbarous philosophy" that had "rudely torn off . . . all the decent drapery of life" (Burke, 1955, p. 87). About all both schools could agree on was that philosophe ideas had prepared the way for, if indeed they had not been the chief cause of, the French Revolution.[4]

Tocqueville analyzed the development of rationalist thought up to his time by distinguishing four different stages. The first stage, which occurred in the sixteenth century, focused on the relationship of man to God; religious reformers "subjected the dogmas of ancient faith to reason," while still maintaining a belief in the Deity (pp. 430-431). The second stage, a development of the seventeenth century, saw the application of this way of thinking by Bacon and Descartes to the realms of natural science and philosophy (metaphysics); these philosophers consciously defined the mode of thought known as "method" and launched a philosophic project that "destroyed the dominion of tradition and upset the authority of masters" (p. 431). The essence of method was a reliance on individual reason, in the light of experience, as the standard for evaluating truth, rather than prejudice, revelation, or tradition (pp. 429, 431).

It remained, however, for a third stage to extend the principles of method to the social and political spheres, turning the world upside down and spreading in the public mind a mode of thinking "by which all ancient things could be attacked and the way opened for everything new" (p. 431). This approach was popularized by the eighteenth-century thinkers, and in particular by the French philosophes and economists (p. 431; *OR*, pp. 158-169). The key to the philosophe position was the idea that "what was wanted was to replace the complex of traditional customs governing the social order of the day by simple, elementary rules deriving from the exercise of human reason and natural law" (*OR*, p. 139). Tocqueville was aware, of course, of other strands of contemporary rationalist political thought that were not as radical or abstract as the French school, and he was personally very appreciative of certain English rationalists like Hume (*OR*, pp. 152-154). Yet his purpose here was not to survey the thought of different writers, but to sketch the core principles of the ideas that had helped to shape the political cultures of his day. In Tocqueville's view, the philosophe school had clearly exercised the greatest impact upon the public mind, becoming "the common coin of thought" throughout much of Europe (p. 431).[5]

Under the influence of the philosophes, French writers of the next generation extended their mode of thinking to "an unrestrained passion for generalizations" (p. 438). This marked yet a fourth stage in the evolution of rationalism contemporaneous with Tocqueville. "No writer . . . is satisfied with an essay revealing truths applicable to one great kingdom, and he remains dissatisfied with himself if his theme does not embrace the whole of mankind" (p. 438). French thought, which in contrast to English thought brought almost no practical experience to bear in writing on politics, was characterized by a "literary spirit" largely devoid of any contact with reality (*Rec,* p. 84; *OR,* p. 148). It built abstract, utopian models, ignoring the real world and real constraints. In France at least, this "literary spirit" came to shape the public's intellectual dispositions: "It was this vision of the perfect state that fired the imagination of the masses. . . . Turning away from the real world around them, they indulged in dreams of a far better one and ended up by living, spiritually, in the ideal world thought up by the writers" (*OR,* p. 146; cf. p. 670, *Rec,* pp. 78-79).

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[4] The idea of studying Tocqueville's thought as a response to these two schools was suggested by J. P. Mayer (1960, pp. 111-112), who also noted that the influence of Burke on Tocqueville has never been sufficiently explored in the scholarly literature. For my treatment of rationalism, I also rely heavily on the ideas of Peter Augustine Lawler, Edward Gargan (1965) and Pierre Manent (1982).

[5] The account of rationalism here is presented from Tocqueville's point of view. An account from the outside would also be helpful, and one reader of this article suggested distinguishing between an analytic rationalist tradition, which went from Bacon and Descartes through Hume, Smith, Montesquieu, and Tocqueville, and a rationalist tradition, in which the thinker adopts the perspective of an omniscient observer, which would include Saint Simon and Hegel.

By the end of the eighteenth century, what passed for rationalist thought was something very different from the original rationalist method identified by Bacon and Descartes and later pursued by Hume and Montesquieu. Whereas rationalism had begun as an inductive or empirical mode of thought characterized by the careful movement from the particulars of experience to generalizations, it evolved by the end of the eighteenth century into an abstract and deductive mode of thought in which authors fabricated their own internal systems and then sought to impose them on reality. Tocqueville regarded this shift from an empirical mode of thought to "a passion for generalization" as a clear perversion of the original spirit of rationalism. All that remained was a suspicion of tradition, and even this had been transformed into an irrational prejudice against the old. Although Tocqueville did not object to the spread of all general ideas, and in particular endorsed the idea of the rights of man, he was convinced that the mental habits inculcated by modern rationalist writers were destructive of the cause of liberty. It was only in America that rationalism in its original, empirical form was practiced in daily politics: "America is the one country in which the precepts of Descartes are least studied and best followed" (p. 429).

Traditionalism received a less systematic treatment from Tocqueville, probably because of its more recent origin. Although Tocqueville was thoroughly familiar with the precepts of this school from the thought of French writers like Joseph de Maistre, he apparently did not trace these ideas to their source until making an in-depth study of Burke's thought during his visit to England in 1833.[6] Tocqueville ultimately disagreed with the traditionalist outlook, but he shared many of its points of criticism of philosophe rationalist thought, and among his contemporaries he clearly directed his works to moderates of the traditionalist persuasion, hoping to change their minds (pp. 16, 17).

Although the traditionalists of Tocqueville's time ranged across a broad spectrum of opinion, their diagnosis of the evils of philosophe rationalism contained a common theoretical position. Traditionalists rejected abstract, metaphysical thought in politics and dismissed a priori reasoning. Burke (1955, p. 69), for example, regarded political science as a "practical science" and emphasized the need to view political phenomena in terms of concrete circumstances and particulars. Traditionalism thus presented itself partly as a defense of the realm of prudence for the statesman against the misinformed intrusion of speculative philosophers. In this respect, traditionalism was consistent with the pragmatic character of original rationalist thought, which counseled making judgments on the basis of experience. But traditionalism went far beyond a reliance on this form of reason in its assault on rationalism.

First, traditionalists attacked metaphysical speculation, and sometimes reason itself, in the name of a veneration for the old and familiar. They upheld the "prejudices" of the community and glorified an "instinctive patriotism" (p. 235). For the traditionalist, Tocqueville explained, "the idea of evil is indissolubly linked with that of novelty" (p. 17).

Second, and following from this depreciation of reason, traditionalists celebrated the heritage of each nation as it emerged as the product of the particular and accidental forces that shaped its development. According to traditionalists, historical bonds of union formed the only sound basis for community, not individual reason or the individual's calculation of interest (p. 236). Each nation's constitution was the product of—and ought to be bounded by—its particular historical development. Beginning from this "organic" perspective on society, traditionalists viewed the homogenizing effects of universalistic ideas with profound suspicion. Universal ideas eroded the basis of community at the same time that they destroyed the charm of diversity in the world.

Finally, traditionalists strongly supported religious faith, especially as it manifested itself in the particular religious institutions of each society. Traditionalists identified speculatist philosophy with irreligiosity and sought to make common cause with the faithful by contending that religion was one part of an integral world view being attacked by rationalist ideas (*OR,* p. 154).

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[6] For the influence of Burke on French thought, see Nisbet (1970, p. 48) and Reardon (1975, p. 22). Burke was also highly influential in German conservative thought, which Tocqueville studied directly and learned about through Gobineau (Chapman, 1967, pp. 4-12).

All of these properties of traditionalism can be found in Burke's writings, but they received a simpler, and more extreme, expression by Joseph de Maistre:

> [Man's] cradle should be surrounded by dogmas; and when his reason awakes all his opinions should be given him. . . . Nothing is more vital to him than prejudices. . . . Religion and political dogmas, mingled and merged with each other, should together form a general or national mind sufficiently strong to repress the aberrations of the individual reason. (Reardon, 1975, p. 24)

After sketching the development of these two schools of thought up to his day, Tocqueville turned to their likely evolution and to their consequences on political culture. Although each school claimed in its own way to support liberty, Tocqueville contended that both ultimately denied that individuals could control their own destiny and therefore eroded a fundamental belief that supported liberty. To maintain liberty, Tocqueville argued, requires more than the acceptance of an idea of rights and certain favorable institutional arrangements, however important each of these may be. Liberty requires a metaphysical foundation in certain views respecting historical causality and free will. It depends on the fact that people can choose and that their choices can make a difference, and on the fact that people *believe* that they can choose and that their choices can make a difference.[7] Without this belief, people lose their "will" to choose and can see no reason to support the idea of rights or legal institutions designed to protect liberty (p. 705). It was precisely this belief in man's free will, according to Tocqueville, that rationalism and traditionalism called into question.

Philosophe rationalism began with the seeming assertion of boundless human choice: "It has been said that the character of the philosophy of the eighteenth century was a sort of adoration of human intellect, an unlimited confidence in its power to transform at will laws, institutions, customs" (*OR,* p. 281). Man can make the world according to the dictates of reason, overthrowing the tyranny of tradition and accident. Yet in its ever-expanding quest for universal principles of explanation, philosophe rationalism led paradoxically to the negation of the conditions for choice. The search for general and universal causes ended by positing the source of human activity not in the decisions of particular individuals, but in mankind as a whole or, beyond, in some immanent force in nature or history. Tocqueville labeled this mode of thinking "philosophic pantheism." By tracing the springs of human activity to abstractions like mankind or history, this doctrine rendered human choice meaningless. Choices for mankind or societies, Tocqueville argued, can only be made by individuals on behalf of the species; the species itself cannot choose. Once one posits the power over human affairs in the species, that power lies with no one in particular and hence with an invisible system that operates on all: "Individuals are forgotten, and the species alone counts" (p. 451). With vast, general causes thought to be controlling their destiny, people lose all will to assert themselves, and they become weak and phlegmatic.

If philosophe rationalism carried to the extreme leads to one form of denial of human choice, traditionalism carried to the extreme leads to another. The result is equally paradoxical in light of the traditionalists' expressed goal of preserving a realm of discretion for the statesman. Yet by depreciating reason and making the heritage of each society both the cause and standard for its regime, traditionalists implied that there were no fundamental choices; all was as it had to be, or nearly so. The protection traditionalists sought to gain for the statesman in the realm of prudential decision making was thus bought at the cost of denying human choice on fundamental questions. "Each nation," in the traditionalist view, "is inexorably bound by its position, origin, antecedents, and nature to a fixed destiny which no effort can change . . .[it is] the product of preexisting facts, of race, or soil, or climate" (pp. 705, 496). Following Tocqueville, John Stuart Mill gave his classic formulation of the traditionalist position: "[Governments] are a sort of organic growth from the nature and life of that people: a product of their habits, instincts, and unconscious wants and desires, scarcely at all of their deliberate purposes" (1958, p. 4).

Traditionalism, then, leads to a doctrine of historical inevitability, although not of the sort with which we are most familiar today. Ever since Hegel, and especially Marx, doctrines of historical inevitability have tended to assume the form of universalistic theories in which the forces at work in history are said to include all of mankind. This post-Hegelian form of historicism folds the universalistic ideas inaugurated by the philosophes into an inevitable movement of *world* history in which all are swept along in accord with some plan that flows either from the movement of Mind or material forces. But doctrines of historical inevitability were originally an outgrowth of traditionalism, and their source can be traced to Burke. This earlier form of historicism sought to demonstrate that the cause of human societies lay with the particular and accidental forces that operate on each nation (or *volk*).[8] Although Tocqueville glimpsed the possibility of a universalistic historicism and advanced one such mild variant of his own, he identified the historicism of his day mainly with those who sought to explain the fate of *particular* nations or peoples.

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[7] In his correspondence with Gobineau, who contended history was the product of racial characteristics, Tocqueville told his friend, "I believe that [your doctrines] are probably quite false; I know that they are certainly very pernicious" (*ER,* p. 227). Tocqueville judged an idea by the dual criteria of its "philosophic merits" and "its moral and political effects" (*ER,* p. 232).

Notwithstanding their attempt to defend rank and hierarchy, traditionalist doctrines of historical inevitability represented a democratic mode of thought in the sense that peoples, races, or climates, but not individuals, control historical development. While Tocqueville thought that traditionalist historicism might preserve a certain diversity in the world among nations or races, he believed that it was no less hostile than philosophic pantheism to the idea of human choice. It left man with the view that his actions could make no difference and that "societies unconsciously obey some superior dominating force," which was precisely the belief that prepared the way for despotism (p. 495). By extending Tocqueville's analysis, one can glimpse in this organic variant of historicism, as transformed by the atheism of Nietzsche, the seeds of the radical "democratic" Right (fascism), just as it is possible to see in rationalism, as modified by Marx, the grounds of the radical "democratic" Left (communism).

Thus rationalism, by leading to philosophic pantheism, and traditionalism, by leading to historicism, deny free will. To promote the beliefs that sustain liberty, Tocqueville sought to free man from the sway of these ideologies. His alternative, the new political science, presupposed a significant, although not unlimited, degree of choice: "One may say that it depends on us whether in the end republics will be established everywhere, or everywhere abolished" (p. x). Political science was designed to help man choose well, which demanded knowledge of the true historical situation and the capacity to face reality, avoiding the mistakes of the rival schools of alternatively exaggerating and underestimating the possibilities that were available.

Against the philosophe's excessive pride that all societies could be re-made according to the simple principles of reason, Tocqueville countered with the claim that regimes were still bound by the particular experiences of each nation. Despite the homogenizing forces set in motion by growing equality and the spread of modern thought, the political culture of each nation retained certain distinct characteristics, and Tocqueville certainly had no desire to impose uniformity for its own sake. Insofar as political science might assist those who directed society, it was not by prescribing precise models that should be adopted in the same fashion in every nation, but by illustrating general principles that should be applied in their own way as far as circumstances within each nation would permit: "The laws of the French republic can be, and in many cases should be, different from those prevailing in the United States . . . [L]et us adopt the principles rather than the details of her laws" (p. xiv).

Against the traditionalists, Tocqueville countered with a twofold argument to meet their two very different (and conflicting) assumptions. On the one hand, traditionalists virtually denied that there could be a general science of politics that could assist legislators. As the world, in the traditionalists' view, is governed by contingencies and accident, and as constitutions are the product of particular cultures, the best the legislator can do is modify a constitution according to standards that emerge from understanding a nation's particular genius. According to Tocqueville, this view underestimates the degree of choice and the role that a general science of politics might play in informing the tasks of legislating and constitution making.

On the other hand, traditionalists assumed that the movement toward equality of conditions and away from a respect for hierarchy and forms could be thwarted by the development of a counterideology. For Tocqueville, this option was closed: "Burke did not see . . . that there could be no question of putting the clock back" (*OR*, p. 21). Although traditionalists purported to look at facts, they refused to acknowledge that the movement toward the social state of democracy and the penetration of society by some form of rationalism were historical facts in their own right that could neither be denied nor reversed. In his Introduction to *Democracy in America,* Tocqueville advanced a mild variant of universalistic historicism that challenged traditionalists to see that the real choice lay not between the old order and democracy, but between democratic despotisms and democratic republics (Zetterbaum, 1967). Given the traditionalists' expressed concern for liberty, they might, when faced with this choice, be persuaded to become adherents of the new political science.

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[8] For historicist accounts that see a movement or plan for the whole world, see Hegel's *Philosophy of History* and Marx's and Engel's *Communist Manifesto.* For the origin of historicist thought in Burke, see Strauss (1953, pp. 294-321) and Pangle (1973, pp. 192-193). Although Burke shied away from anything like a pure doctrine of historical inevitability, the latter tendency became much more pronounced in those who drew their inspiration from his thought. It is also possible to trace the seeds of Burke's emphasis on history and circumstance in Montesquieu, whom Burke considered "the greatest genius which has enlightened this age" (Chapman, 1967, p. 23). But the emergence of history as possibly the most important standard is far more evident with Burke and with those who drew directly on his thought.

## The New Political Science

Tocqueville's new political science combines generals and particulars to form a body of knowledge designed to promote the cause of liberty in modern times. It can thus be characterized not only by its normative goal, which makes it quite different from modern political science, but by its distinctive epistemological foundation. Tocqueville's political science falls into a middle ground between rationalism, which extended general ideas to the point of eliminating any prudential consideration of specific circumstances, and traditionalism, which virtually denied the existence of any general ideas that could inform or instruct prudential judgment. But it is much easier, Tocqueville tells us, to imagine the existence and utility of such a middle ground than to put it into practice.

The central problem for a science of politics concerns the epistemological status of "general ideas." All general ideas, Tocqueville maintains, distort reality in some degree by comprehending the distinctness of different objects under the same concept. Human thought creates a world of partially fictitious wholes that assigns greater similarity to things than actually exists in the real world: "General ideas do not bear witness to the power of human intelligence but rather to its inadequacy, for there are no beings exactly alike in nature" (p. 437). It was this difficulty, among other concerns, that led Burke and other traditionalists to doubt the worth of a theoretical political science. For Tocqueville, however, the proper response was not to deny the value of general ideas, but rather to recognize their limitations and avoid using them in an extreme or abstract fashion. Without recourse to general ideas, no science, indeed no advancement in human understanding, would be possible, for man "would soon be lost in a wilderness of detail and not be able to see anything at all" (p. 437; Gargan, 1965, pp. 42-44).

Political science is the careful construction of knowledge in which general ideas are founded not on abstract principles that flow elegantly from self-contained intellectual systems, but on the painstaking study of particular cases and the "slow, detailed, and conscientious labor of the mind" (p. 440). "One man . . . notices a fact and another conceives an idea; one man finds a means and another discovers a formula" (p. 268; cf. p. 438). Yet even this knowledge, because it relies on general ideas, is imperfect and does some violence to the understanding of particular circumstances. This difficulty creates an inevitable—although not unbridgeable—gap between the statesman and political scientist, "For while it is sometimes necessary to brush rules of logic aside in action, one cannot do so in the same way in treatises, and man finds it almost as difficult to be inconsistent in his words as he generally finds it to be consistent in his actions" (p. 20).[9] Tocqueville prepares his readers for this problem by warning them that he must sometimes bring his own ideas "to the verge of the false and impracticable," although he never consciously "fitted the facts to opinions instead of subjecting opinions to the facts" (pp. 20, 19).

A perfectly accurate understanding of reality, Tocqueville tells us, cannot be attained, for it would require a humanly impossible method that could somehow reap all the benefits of generalizing while avoiding all of its defects. Only God can employ such a method, for He alone "can survey distinctly and simultaneously, all mankind and each single man" (p. 704; cf. p. 437). Science does the best it can using the limited tools of human intelligence to emulate this God-like standard.

The thought of philosophe intellectuals, by contrast, ignores completely this God-like standard by attempting to subsume more and more of "reality" into abstract general ideas without regard to the existence of particulars. Instead of minimizing the defect inherent in the structure of human thought, the philosophe intellectual exacerbates it. If all that were involved in this process was the activity of some isolated individual interpreting the world, the danger would be slight. But in fact philosophe thought was designed expressly to create a popular political program, the aim of which was to eliminate complex customs and traditions and reshape reality in accord with the "rules of logic and a preconceived system" (*OR,* p. 147). In line with this program, the rationalists of Tocqueville's day tended to support the powers of the largest unit of authority, the central government, no matter whether it was a strong, "enlightened monarchy or a centralized republic" (pp. 97, 723-724). Philosophe intellectuals saw the central government with its centrally directed administrative apparatus as one of the chief instruments for "recasting [a nation] in a given mold, of shaping the mentality of the population as a whole and instilling the ideas and sentiments [of the intellectuals] into the minds of all" (*OR,* p. 162).

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[9] I have changed Lawrence's translation in this passage.

Even more important for Tocqueville than the philosophe's program for change was the consequence of its framework of thought on the mental habits of society. By its inattentiveness to particulars, philosophe thought promoted a disregard for diversity and a partiality for uniformity. By contrast, the new political science, in its concern for facts and practice, implicitly encouraged a respect for diversity. Thus, apart even from a consideration of the content of their programs, the new political science embraced a mode of thought more favorable than philosophe rationalism to a political culture that supported liberty. In his most general and revealing statement on the consequences of rationalist general ideas, Tocqueville wrote:

> To force all men to march in step toward the same goal—that is a human idea. To encourage endless variety of actions but to bring them about so that in a thousand different ways all tend toward the fulfillment of one great design—that is a God-given idea. The human idea of unity is almost always sterile, but that of God is immensely fruitful. Men think they prove their greatness by simplifying the means. God's object is simple but His means infinitely various (pp. 734-735).

Rationalist intellectuals were either ignorant of the distortion of reality in their thought or else chose from ambition to disregard it. Yet far from reducing their influence on society, this defect tended to increase it, for "what is merit in the writer may well be a vice in the statesman, and the same things that often make beautiful books can lead to great revolutions" (*OR,* p. 147; cf. *REC,* pp. 84-85). Here Tocqueville suggested the central problem of the intellectual in politics: theoretical constructs that are a false reflection of reality are apt to have a much greater impact on the public mind than those that present a true picture of reality. The products of a "literary spirit" result in "beautiful" works, whereas the products of a scientific spirit must sacrifice beauty for accuracy.

Traditionalists had correctly identified this homogenizing tendency of modern philosophe thought as its most characteristic and dangerous trait. But while sharing their concern, Tocqueville rejected their standards and doubted the efficacy of their methods for combatting philosophe ideas. Unlike the traditionalists, Tocqueville upheld reason at the same time that he attacked the reason of his contemporaries and defended the intellect at the same time that he attacked the modern intellectual.

The underpinnings of Tocqueville's new political science were not entirely original. Concern about the consequences of philosophe thought and about the need to blend the universal and the particular can be found in the works of the writers whom Tocqueville studied most closely (Rousseau, Montesquieu, and Pascal). Yet the fact that Tocqueville owed a debt to other thinkers, and especially to Montesquieu, does not reduce the significance of his call for a new political science, for on Montesquieu's own grounds political philosophy must be "presented not only in the form of a philosophic treatise but simultaneously in the form of a tract for the times" (Pangle, 1973, p. 305). Just as the principles of political science must be adapted to each country, so the principles of political philosophy must be adapted to each new age (p. 543). In the brief period between Montesquieu's death and Tocqueville's publication of *Democracy in America,* modernity had undergone one of its greatest transformations. Tocqueville's claim to originality (if his words can be read in those terms) was not for a new political science per se, but for a "new political science . . . for a world itself quite new." Tocqueville's achievement lay, as Mill had said, in writing the "first philosophic book . . . on Democracy."

## Political Science as an Active Force in the World

Given the complicated character of his new political science, one immediately wonders how Tocqueville hoped to make it into an active force in modern society. Although the goal of political science—reconciling liberty and democracy—was not hostile to the general spirit of modern times, its complexity left it without widespread appeal. It could, of course, be taught directly to the few who studied his works, but Tocqueville was under no illusions that it could become a popular ideology that could, like philosophe thought, "fire the imagination of the masses." "Generally speaking, simple conceptions take hold of people's minds. A false but clear and precise idea always has more power in the world than one which is true but complex" (p. 164).

Since political science could not win a public war of ideas against philosophe ideology, Tocqueville employed a more indirect strategy based on encouraging certain mental habits in society that would be compatible with, if not necessarily exactly the same as, the lessons taught by political science (p. 287). These mental habits were characterized less by any particular content (in the sense of a set of ideas outlining a program or agenda) than by a way of thinking that people would employ. Tocqueville did not seek to fight ideology with ideology, not only because such a battle would have been futile, but also because a society directed by ideology (and intellectuals) could not, in his view, remain a free regime.

To be more specific, Tocqueville's strategy involved showing those who direct society what kinds of social and political institutions could limit the intellectuals' influence over public opinion and help citizens to learn to reason from particulars to generals rather than from generals to particulars. Under Tocqueville's strategy, public opinion would form more along the lines of the American than the French model. The influence of ideas produced by intellectuals at the "top" would diminish, whereas that of other sources of authority (e.g., political leaders and lawyers) and of opinion generated from the "bottom" would increase. Opinion generated from the bottom was a reflection of democratic sentiments and concrete interests as these were modified by the knowledge and experience gained by citizens involved in the task of self-government.

In this section, I shall examine Tocqueville's application of the practical aspects of the new political science to the press, the legal system, and local government, noting how he employs his distinctive standard of judging institutions by their effect on mental habits. In each case, Tocqueville illustrates how American institutions, especially when compared to those in France, serve to reduce the influence of general ideas and to promote reasoning from particulars to generals. Tocqueville's method of analysis here is slightly unexpected given his starting point of studying America in order to see the picture of democracy. Yet American political culture, as noted, does not follow democracy's natural path of development. In the instances discussed below, France becomes the picture for certain democratic tendencies in their pure (and dangerous) form, whereas America becomes the (partial) model for how these tendencies can be corrected in a framework that is consistent with democracy.

## The Press[10]

Tocqueville's discussion of the press is the most striking case of his effort to limit the influence of modern intellectuals. In line with the position of most *philosophe* thinkers, Tocqueville was a staunch advocate of a free press, arguing that citizens in free regimes need a free flow of information to govern themselves and that the possession by government of the power to regulate the press could lead by stages to despotism. Yet unlike the philosophe thinkers, who saw the press as a perfect vehicle for spreading their version of enlightenment to the public, Tocqueville preferred an arrangement of the press that would limit the intellectual's influence.

As an independent source of power over public opinion, the French press had no equal. The "vital part" of its newspapers was devoted to political discussion, and the "spirit" of French journalism demanded a "lofty and eloquent way of arguing about the great interests of state" (pp. 183, 185). French journalists were highly esteemed, as the relatively small number of newspapers were concentrated in Paris and attracted talented and cosmopolitan writers. It was with the aid of the press that intellectuals were able to spread their thought from the center to the peripheries and to enlighten the provinces with the latest and most advanced ideas.

The American press could hardly have been more different. Newspapers were filled with advertisements, and the presentation of the news lacked the kind of "burning argument" and lofty commentary that was commonplace in France (p. 184). There was an extraordinarily large number of newspapers in America, which were widely dispersed throughout the nation. The effect of this arrangement was to divide and limit the American press's influence, in comparison with that of the French press. In addition, American journalists were generally of low educational status and possessed only modest talents. When they expressed political viewpoints, they did so not by adopting an intellectual perspective and hiding their appeals to passions in philosophic language, but instead by making crude appeals to lower passions, often "attacking people [and] revealing their private lives and vices" (p. 185). Although American journalists won a wide audience by these techniques, the public, and certainly its more enlightened elements, sensed the narrow spirit of their inquiry and had little respect for journalists as a group.

Even though the press in America was less powerful than in France, its influence was still very great: "[a]fter the people, the press is nonetheless the first of powers" (p. 186). Yet it was not journalists as opinion makers who held most of this power; rather, it derived from the newspapers' role in communicating ideas among groups in society, such as political parties and associations. The American press therefore contributed to the formation of public opinion by political associations rather than by intellectuals, allowing opinion to build from the bottom to the top, rather than from the top to the bottom (p. 520).

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[10] In this section, I borrow freely from the excellent discussion of Tocqueville's thought by Bourricaud (1980, pp. 37-69).

By any criterion of literary or journalistic excellence the American press was inferior to that in France, and Tocqueville never doubted that the "deplorable abuse of thought" that took place in the American press would adversely affect "the taste and morality" of the American people (p. 185). But the harm done to American mental habits in this respect was more than offset by the benefit derived from blocking a portal of access for the intellectuals' general ideas. Tocqueville was quite willing to sacrifice sophistication and even to admit a degree of coarseness into society as the price for protecting citizens from the thinkers who sought to run their lives.

## The Legal Profession

Tocqueville's discussion of the legal profession provides a second example of his effort to limit the power of intellectuals and general ideas (p. 267; cf. p. 185). Tocqueville sought to promote the legal spirit in society to serve as a counterweight to the literary spirit, and lawyers as a counterweight to intellectuals.[11] The legal way of thinking, with its roots in the judicial process, has something in common with the empirical strain of early rationalist thought: "[I]t pronounces on particular cases and not on general principles" (p. 160). Judges and lawyers are trained to reason from particulars to generals and therefore develop an affinity for order and forms that leads them as a rule to shun momentary popular passions. To the extent that the legal profession influences the thinking of society as a whole, it imbues the citizenry with some of their mental habits. Tocqueville, for example, considered the importance of the institution of the jury as much in terms of its impact on mental habits as on its efficiency as an instrument of justice.

After sketching the general spirit of the legal profession, Tocqueville turned to the particular spirit of French and Anglo-Saxon lawyers. Although lawyers in both nations reason from particulars to generals, French lawyers, following continental theories of jurisprudence, analyze particulars in the light of general principles, whereas Anglo-Saxon lawyers, working in the common law tradition, consult "the law of precedents" and consider particulars in light of "the legal judgments of their fathers." "The first thing an English or American lawyer looks for is what has been done, whereas a French one inquires what one should wish to do." The spirit of French law is more abstract, as it is based on a "whole system of ideas," whereas that of English and American law is more historical (p. 267).

The spirit on which Anglo-Saxon law rested antedated not only philosophe thought, but the emergence of rationalism. It incorporated many characteristics of traditionalism, and Tocqueville's discussion of English and American law virtually reads as a commentary on traditionalist thought. English and American lawyers proceed with a "superstitious respect for all that is old" (p. 269) and value laws "not because they are good but because they are old" (p. 268). With their expertise resting on an "obscure" tradition, they relate to the public "somewhat like the Egyptian priests, being, as they were, the only interpreter of an occult science" (p. 268). English lawyers will introduce change, but only by hiding the fact and going to "absurd lengths" to avoid admitting to the "crime" of being an "innovator" (p. 268). The character of English law is decidedly organic—a point Tocqueville emphasizes by employing the metaphor of a "trunk of an old tree" on which lawyers continually graft "the strangest shoots" (p. 268).

Although Tocqueville appreciated much in the traditionalist character of Anglo-Saxon law, his ironic tone and admission that this system of legal thought was filled with "defects" leave no doubt that he considered traditionalism to be unsuitable as the first principle for the entire government (p. 268). In any case, its merits aside, the Anglo-Saxon legal spirit could not serve openly as the foundation for a modern society, as its premises were out of step with rationalist standards of legitimacy. Anglo-Saxon lawyers could "raise no banner of their own" and won a measure of influence not by openly defending their principles, but by insinuating their spirit into society, "penetrating each component class and constantly working in secret upon its unconscious patient" (p. 270). In spite of these reservations, however, Tocqueville thought that the Anglo-Saxon legal spirit provided a salutary corrective to the excesses of certain democratic tendencies, moderating the citizenry's fascination with novelty and limiting its susceptibility to the appeal of the intellectual's general ideas. The legal spirit in the United States served in its context to promote the ends of the new political science, even though it was not grounded strictly on the mental habits of early rationalist thought (p. 268).

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[11] Tocqueville provides a rough definition of spirit *(l'esprit)* as, in any particular case, "certain ways to which all must conform . . . the sum of these common ways is called a spirit; there is the spirit of the bar, the spirit of the court" (p. 185). We are obviously also meant to think of Montesquieu's usage of the term.

## Communal Government[12]

Tocqueville's discussion of communal liberty and citizen participation in local governments is the best known example of his application of the principles of the new political science to practical affairs. It is important, however, to avoid confusing his ideas on citizen participation with an endorsement of populism in national politics (p. 253). Tocqueville was a staunch opponent of direct democracy in national politics and a strong defender of the American founders' goal of establishing representative institutions that preserved broad discretion for deliberation and statesmanship. His idea of participation emphasized activities in matters falling within the sphere of the citizens' own experience, not in affairs of state such as foreign policymaking. The importance of participation was based as much on the mental habits promoted by the activity of deciding as on the content of what people decided (pp. 511, 694).

By taking part in political affairs, citizens learn the mental habits of the pragmatic strain of rationalism. These habits serve to reduce the influence of general ideas and increase the weight of experience in the formation of public opinion: "If, then, there is a subject concerning which a democracy is particularly liable to commit itself blindly and extravagantly to general ideas, the best possible corrective is to make citizens pay daily practical attention to it" (p. 442). For citizens to become enlightened in more than a cosmetic sense depends, in fact, on their being able to complete their literary education by participating in the practical task of governing: "True enlightenment is in the main born of experience, and if the Americans had not gradually grown accustomed to rule themselves, their literary attainments would not help them much toward success" (p. 304).

Local government also functions for Tocqueville as a major "secondary power" that stands between the individual and the central state (p. 676). Tocqueville seldom discussed liberty without also discussing the practical means by which it could be secured, and these included the possession by citizens of a sense of their *power* to defend their rights. Secondary powers in Tocqueville's thought serve as rallying points for citizens to resist the overwhelming weight of central authority. Yet secondary powers are threatened in democratic times by the many forces tending towards concentration of authority in central governments, and it is only by great effort and intelligence that they can be maintained: "In the dawning centuries of democracy individual liberty and local liberties will always be products of art; centralized government will be the natural thing" (p. 674).

Of the secondary powers Tocqueville identified—civic associations, political associations, and business enterprises—he considered the commune to be the most durable, for it alone rested on a "natural" foundation (although it too could only be maintained by "art"). The commune is "natural" in the sense of being a form of social organization that grows organically rather than being consciously constructed: "[It] is the only association so well rooted in nature that wherever men assemble it forms itself. . . . [M]an creates kingdoms and republics, but communes seem to spring directly from the hand of God" (p. 62). Tocqueville introduces here a key distinction between the "natural" (in the sense of the organic) and the conventional (that which man makes by the application of his own intelligence). Tocqueville's final standard, it should be observed, is not the natural in the organic sense, but rather "nature" or "natural right" as discovered conventionally by human reflection on the world and its possibilities (p. 237). In contrast, however, to the fabricated "human ideas" propagated by philosophe intellectuals, Tocqueville's understanding of natural right never departs entirely from the natural in the organic sense, in part because the failure to recognize organic needs leads to social dysfunctions. A satisfactory modern regime must combine a man-made principle of rule with respect for the natural foundations of community that are found in the communes.

The conflict Tocqueville identified between the standards of philosophe rationalists and the standard of the natural (organic) is played out on the plane of practical politics in the conflict over the role of local governments in the modern state. The commune develops outside the sphere of human contrivance and derives much of its operating spirit not from abstract theory or ideology, but from custom and practical experience. Philosophe intellectuals, who rely for their power on units of central opinion formation and their alliance with central authority and centralized administration, regard communal liberty as an enemy of their designs. They want to impose their standards on the commune and thus run roughshod over communal custom and local experience: "A very civilized society finds it hard to tolerate attempts at freedom in a local community; it is disgusted by its numerous blunders and is apt to despair of success before the experiment is finished" (p. 62). Here Tocqueville depicts the characteristic vice of modern intellect, which is an absence of forebearance and an impatient impulse to do things for people rather than to let them act for themselves. Philosophe rationalists prefer to impose the right opinion in every case rather than inculcate the mental habits that would allow people to decide for themselves (and therefore inevitably to make mistakes).

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[12] Lawrence sometimes translates *commune* as township, where Tocqueville was suggesting a connection between the American local community and local communities found everywhere. When referring specifically to the American local unit of government, Tocqueville often included the English word "township" in parentheses. I have substituted "commune" for "township" in some of the translations in this section.

The question of where to draw the line between central and communal authority was for Tocqueville a complicated matter that depended on such considerations as security needs, the stage of economic development, and the degree of enlightenment in the communes. Although not a dogmatic opponent of all "intrusions" of the central authority in local affairs, Tocqueville nonetheless expressed a strong preference, all other things being equal, for decentralization.[13] In the northern states of the United States, Tocqueville found his practical ideal of the modern commune. Free of feudal customs, these localities had developed a new tradition based on equality and the exercise of pragmatic rationalism in politics. These communes were enlightened, but they were still far from being the centers of sophistication adored by philosophe intellectuals. Tocqueville had no hesitation, however, in defending them, as his goal was not a society directed by the most advanced discoveries of modern thinkers, but one that allowed free and enlightened citizens to practice the art of self-government.

Tocqueville's comparison of opinion formation in France and America risks slipping at times into an ideal-type analysis in which the United States, in order to be contrasted with a nation influenced by philosophe rationalist thought, is presented as a society devoid of intellectual reflection and successful only because of the unconscious operation of certain geographical and historical advantages. In fact, while Tocqueville pointed to many of the fortunate circumstances that contributed to America's success, his analysis makes clear that the establishment of the Constitution in the 1780s was a remarkable achievement of political sagacity. America was adverse to "general ideas," if by general ideas one means the sort of ideas generated by the philosophes and their opponents. But the American founders were certainly not adverse to the study of the first principles of politics, and important elements of the American public grasped the principles of science on which they had based their work (pp. 113-114, 152, 164).

Although Tocqueville had nothing but praise for the intelligence of America's founders, it is nevertheless significant that he did not consider any American to have been a "great writer" on politics (p. 301). The understanding of what made America work was only partly contained in the thought of *The Federalist,* which, because it concentrated on the principles for establishing a *national* government, provided an incomplete account of state and local governments and the fundamentals of American political culture (p. 61). Tocqueville's analysis in effect combined the concerns of the Federalist and the small republic or anti-Federalist traditions in American political thought by blending certain attributes of the small republic—or at any rate worthy facsimilies thereof—with the qualities needed to sustain a great, modern commercial republic (p. 287). The result, he evidently thought, was a more complete political science than that of any American thinker.

## The Synthesis of Rationalism and Traditionalism

To promote liberty in modern times, Tocqueville sought to reduce the influence of intellectuals and general ideas and to instill in citizens the mental habits of the early rationalist method of reasoning from experience and particulars. Although Tocqueville was as critical as Burke of the modern philosophe strand of rationalism, he nevertheless made rationalism, not traditionalism, his principal standard for modern society. He rejected traditionalist notions of prejudice, superstition, and emotion as the foundation for government on the grounds that they were inconsistent with the spirit of modern times and incapable of supporting a regime of equality in liberty.

Tocqueville's project can therefore be described as an effort to rescue the original core of rationalism from its later distortion. Yet this statement needs to be qualified, for Tocqueville thought that rationalist standards, even those derived from the early inductive approach, were not entirely adequate as a foundation for a free and healthy society. Moreover, the distortion of early rationalist thought that took place in the eighteenth and early nineteenth centuries was not, in Tocqueville's estimation, a mere accident, but a logical result of certain flaws and limitations implicit in the original rationalist position. Thus, even to save the original core of rationalism, Tocqueville argued that it had to be supplemented and modified. These changes were based on insights Tocqueville drew from traditionalist principles, although he sought to apply these insights in ways that were compatible with the spirit of rationalism.

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[13] Tocqueville acknowledged the need for greater centralization in Europe than the United States because of the necessity of reforming its feudal mores (p. 90). In the United States, he suggested that the federal government would have a similar role to play with respect to eliminating prejudice against Indians and Blacks (p. 337). In future times, economic conditions would call for a very active role for the federal government as protector and regulator (p. 684). Finally, although Tocqueville clearly preferred as a general rule to strengthen decentralizing trends against the tendencies of democracy toward constant centralization, in the United States he felt that the drift toward decentralization was too strong and threatened the nation's unity and security (pp. 390-393).

The originality of Tocqueville's new political science inhered largely in its synthesis of these two schools. Although predominantly a rationalist and a liberal, Tocqueville sought to integrate certain traditionalist values such as religiosity, community, and spiritedness into the democratic liberal state. These values Tocqueville considered worthy in themselves, but he also stressed their utility. The failure of liberal rationalist regimes to incorporate these values would lead to severe dysfunctions that in the end would threaten their prosperity and freedom. The dual nature of Tocqueville's analysis, at once friendly to liberalism but mindful of its problems, helps to explain his powerful influence on both liberal thinkers and those of the European sociological tradition who have stressed the major difficulties of liberal society.[14]

Tocqueville discussed the central problem posed by the entire rationalist project in his extensive treatment of the nature of intellectual authority in modern times (pp. 429-500). The original rationalist position as it filtered into society held that each person could make up his own mind, using his personal assessment of reality as the standard of truth. But this position, Tocqueville argued, was untenable, for most human beings lack the time, inclination, and capacity to decide on a great many matters of fundamental importance: "Man has to accept as certain a whole heap of facts and opinions which he has neither leisure nor power to examine and verify for himself . . . [s]omewhere and somehow authority is always bound to play a part in intellectual and moral life" (p. 434).

Aristocratic societies were arranged implicitly in acknowledgment of this fact, and people experienced no great problem in receiving opinions from tradition or from those considered wiser. In democratic societies, however, individuals are imbued with the rationalist idea that they can make up their own minds, and, considering themselves fully equal with everyone else, refuse to accept the intellectual authority of any other individual. Nevertheless, sensing their own weakness and needing to resolve matters, they turn to public opinion as the source for their ideas (p. 435). By bowing to no one in particular, they flatter themselves that they have not sacrificed their own judgment. With their minds thus at ease, they accept a source of intellectual authority that is potentially even more powerful than those found in aristocratic societies. Rationalism, which begins from the supposition that each person can make up his own mind, may paradoxically end with a state of affairs in which few think for themselves and in which "public opinion becomes more and more mistress of the world" (p. 435).

The central question in Tocqueville's analysis of democratic society thus becomes who or what forms public opinion. Tocqueville provides two basic answers: first, public opinion may emerge from the interests and sentiments of the people; second, public opinion may be formed by intellectuals. There are differences in the character of opinion as it derives from these two sources, though not as great as one might suppose. The opinion that emerges from the people is coarser and reflects, among other things, untutored desires for material gratification and for some kind of equality (pp. 552, 504); the opinion formed by intellectuals fans the passions for equality and uniformity, dressing them in high-brow philosophic language and endowing them with utopian expectations (*OR,* pp. 158-159). Accordingly, both sources produce opinions that are democratic in content; but the opinion that derives from intellectuals is rigid and dogmatic, whereas that which derives from sentiment is more amenable to modification by political experience and to direction by political leaders (*OR,* p. 142).

To explain why the opinions fostered by intellectuals are democratic, Tocqueville presents a three-part sociology of ideas and intellectuals. First, he argues that only those ideas that are broadly consistent with prevailing social conditions can attract public support (p. 431). Social conditions do not determine the exact content of thought or the ideas of every thinker. But social conditions do suggest certain ways of thinking about reality, which most thinkers then ascribe—often erroneously—to all times and places. For example, given the weakness of individuals in democratic times, most thinkers are led to posit that history is moved by general causes rather than individual efforts (pp. 439, 494-496). More important, social conditions set boundaries to the kinds of ideas that are potentially acceptable. Thinkers who stand outside these boundaries—

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[14] Included on this list would be Emile Durkheim and, in the United States today, Robert Nisbet and Peter Berger.

like traditionalists—are unable in the end to exercise much influence. Thus, although intellectuals may shape the public mind, they can do so only so far.

Second, intellectuals in the modern era are perhaps especially prone to reflecting the common views of their times. Those who become intellectuals are not members of a distinct class having a set of values fundamentally different from the rest of society, but rather emerge from society after having been initially formed by democratic tastes and passions. They employ their thought to give expression to these sentiments and are often little more than the most articulate conveyors of democratic prejudices (pp. 457, 460).

Finally, Tocqueville suggests a narrower and more self-interested reason for the intellectual's adherence to democratic ideas. Like all others in democratic times, intellectuals must earn a living and make their way in the world by practicing a career; they engage in their trade as a vocation, not an avocation (p. 458). As a result, their success depends on how well they can sell their ideas to their contemporaries. The intellectual *qua* intellectual is not accorded deference, but rather can gain influence only by his ability to appeal to a portion of the public and supply it with the kinds of ideas it can grasp. The intellectual is therefore led by ambition and the desire to secure a livelihood to produce ideas that can meet with widespread approval. In content, this means purveying ideas that are consistent with the democratic passions, chiefly equality and uniformity; in style, it means writing in broad generalities, because democratic citizens, needing "sound" opinion on many matters but unwilling or unable to spend time on difficult inquiries, crave general ideas (p. 442).

Although the opinion generated by intellectuals tends to be democratic, it differs, as noted, from the opinion that emerges directly from popular democratic sentiments. Intellectuals create opinions that appeal to the imagination and that move people beyond a common sense calculation of their interest to imagine utopian visions; and even though intellectuals must sell their ideas, they are not (with the exception of the masters of popular entertainment and culture) bound by the immediate views of the mass public. They produce for a more limited high-brow or literary-minded public that is more likely to get excited by their new thoughts (pp. 461, 473). The intellectual's influence over the public mind is exercised by a two-step process in which potentially quite radical ideas are first digested by a literary-minded sector and then by degrees disseminated to the public at large.

If this analysis helps explain the intellectuals' ability to manage opinion in modern society, the problem for Tocqueville was how their influence could be limited by mental habits reflecting the empirical strand of original rationalist thought. For this, Tocqueville thought it better to work with opinion as it emerged naturally from democratic passions and sentiments, educating and moderating it by means of the solutions noted in the last section. Yet for these solutions to work, Tocqueville made it clear that steps would also have to be taken to deal with certain fundamental moral and intellectual problems that rationalism, even in its empirical form, left unresolved. It was here that Tocqueville turned to traditionalist insights.

Tocqueville's most important qualification of a rationalist foundation for society was his assertion of the need for religious belief. Tocqueville stands out as one of the staunchest supporters of religion among the great liberal thinkers, and it is on this point that his break with rationalist intellectuals of his time was most explicit (p. 295; *OR,* pp. 148-157). In matters of belief about religion, as on the question of the rights of man, Tocqueville made an exception to his position against general ideas, or rather took the occasion to clarify his position about the domains of opinion in which general ideas were needed: "General ideas respecting God and human nature are . . . the ideas above all others which ought to be withdrawn from the habitual action of human judgment" (p. 443). If the scope of critical inquiry in these areas were not limited, people would be thrust into an intellectual vacuum leading to despair, paralysis, and eventually susceptibility to false and dangerous pseudo-religions.

By contrast, the philosophes held that people could live without a religious dimension in their lives. But this view, according to Tocqueville, was both false and detrimental to society. People possess a "taste for the infinite," which, if repressed in one form, must surface in another (p. 535). Two results follow from the attempt to destroy religious beliefs. In one case, most people abandon religious faith and immerse themselves in a consuming pursuit of material gratification. But within this secularized society, people experience a gnawing feeling of emptiness, causing some to turn to fanatical religious sects or to mysticism (p. 535). In the other case, people displace their religious sentiments onto politics, thus creating a religion of revolution. This had already occurred in France, where the French Revolution "though ostensibly political in origin . . . assumed many of the aspects of a religious revolution" (*OR,* p. 11). Tocqueville here identified the mindset of the modern intellectual revolutionary—a revolutionary of a "hitherto unknown breed"—who saw no meaning to life outside the political realm, but who, because of this denial, infused politics with all of his repressed spiritual feelings (*OR,* p. 157).

In contrast to rationalism, Tocqueville's new political science recognized the spiritual needs of man as a fact of human existence which a healthy social order must take into account and channel in a constructive way. Tocqueville based his view in part on the traditionalist view regarding man's incapacity to support intellectual doubt, but he did not extend this principle to a reliance on "prejudice" in political affairs. His model for the intellectual foundation for modern society was taken from the United States, where citizens managed to join a belief in God with a pragmatic rationalist approach to truth in other spheres. Although resting on different foundations, religion and rationalism were joined together in America into an intellectual whole, with religion not only coexisting with rationalism, but actually enhancing its role in practical affairs. The limitation of rationalist inquiry on a few metaphysical questions provided spiritual comfort and solace, enabling people to exercise rationalism more effectively in the domain of politics (p. 444, *OR,* p. 153). Hence Tocqueville's famous claim: "I am led to think that if [man] has no faith he must obey, and if he is free he must believe" (p. 444).

In addition to neglecting man's spiritual needs, rationalism in Tocqueville's view promoted the vice of a radical and asocial individualism. According to rationalist tenets, each individual separately judges truth on the basis of his own reason and experience: "[E]ach man is narrowly shut up in himself, and from this basis makes the pretension to judge the world" (p. 430). This leads to a malady that Tocqueville called "individualism," but which, given the modern connotation of that term, might better be called privatism. Privatism has the effect of making individuals ignore all political and social concerns and withdraw into a life preoccupied with the well-being of self and family. It helps pave the way for despotism, which "sees the isolation of man as the best guarantee of its own permanence" (p. 509).

Although Tocqueville argued that people possessed a natural yearning for community, he did not think it was sufficient to withstand the disintegrating impact of rationalist intellectual doctrines and modern social conditions. To integrate man into the community required a mixture of rationalist and traditionalist principles. Unlike traditionalists, Tocqueville thought that the fundamental bond between the individual and community in modern times had to rely on a calculation of interest, not on feelings of patriotism (p. 236). To a certain extent, rationalism contained the remedy of its own defect, for citizens could learn from participating in politics that their private interest would be promoted in the long run by furthering the public interest. As a result of the mental habits formed by this principle of "self-interest rightly understood," citizens would develop a concern for the community as such (pp. 510-511).

Yet the integration of the individual into the community, which is essential to fulfilling man's yearning for a social existence, could not be accomplished without also relying on the effective attachment of individuals to their communities as a supplementary bond: "Every citizen of the United States may be said to transfer the concern inspired in him by his little republic to love of the common motherland" (p. 162; cf. p. 94). Tocqueville's view of the political community thus reflected a synthesis of rationalism and traditionalism. A modern state is not, as certain traditionalists would have it, a mere enlargement of an organic unit. Neither, however, is social organization entirely a human convention created by asocial individuals who contract to form a community, as so many rationalists posited. The two conceptions need to be combined into a view of the state that recognizes individual rights but that also accords a great deal of independence to local governments.

The final difficulty with rationalism, in Tocqueville's view, lay in its inability to account for the deepest source of man's attachment to liberty. Rationalism can help people see their interest in liberty and, through the principle of self-interest rightly understood, lead them to appreciate its utility for producing such goods as wealth and power. Ultimately, however, this utilitarian standard is inadequate. Liberty can be maintained in the final analysis only if people value it as an end worthwhile in itself. This kind of commitment to liberty inheres in the "will" rather than any calculation of self-interest (p. 705). As human "will" falls into the category of a moral sentiment of the heart, it must be evoked by a rhetoric that goes beyond interest and that appeals to traditionalist notions of pride and spiritedness. Tocqueville accordingly urges those who direct democratic society to exhort citizens to have a "higher idea of themselves and of humanity" (p. 632):

> What has made so many men, since untold ages, stake their all on liberty is its intrinsic glamour, a fascination it has in itself, apart from all "practical" considerations. . . . The man who asks of freedom anything other than itself is born to be slave. [The love of liberty] is something one must *feel* and logic has no part in it. It is a privilege of noble minds which God has fitted to receive it, and it inspires them with a generous fervor. But to meaner souls, untouched by the sacred flame, it may well seem incomprehensible. (*OR,* p. 169)

## Conclusion

In analyzing the thought of any major political theorist, it is necessary to begin by making an effort to understand the world in the same terms as that theorist. Yet in Tocqueville's case, to stop at that point would fail to fulfill the spirit of his enterprise. Tocqueville's political science calls on others to continue the project of attempting to reconcile liberty and democracy by engaging in concrete analyses of the specific social conditions and institutions in each era and each regime: "Different times make different demands; the goal alone is fixed . . . the means of getting there ever change" (p. 543).

Here, of course, is not the place to pursue such inquiries, but it is clear that America has experienced fundamental changes since Tocqueville's time in some of the elements shaping its political culture. America's intellectual elite, once shut off from the latest ideas and fashions, now often sets the trend for Western intellectual development. Although American political culture probably continues to remain more resistant to the immediate influence of deductive intellectual thought and "general ideas" than the political cultures of certain European nations, recent studies of American society—especially the literature on the emergence of the so-called "new class"—point to a growing influence of America's intelligentsia in shaping the way people think (Bell, 1973; Bruce-Briggs, 1979).

This increased influence of the intellectual stratum in America has been accompanied by a weakening of the specific counterweights to the rapid transference of "general ideas" throughout society. In comparison to Tocqueville's time, the dominant jurisprudential theories are less traditionbound and more open to influence by contemporary intellectual ideas. The news media have become more centralized, and major journalists, who now enjoy an extraordinarily high status, have developed close ties to intellectual centers. Finally, the political system, at least until quite recently, has become more centralized, with many matters previously considered local now decided at the level of the federal government. America can no longer rely, as it could in Tocqueville's time, on its insularity or the peculiar character of its institutions to escape the consequences of the propagation of intellectual ideas in society. Like the European nations he addressed in the nineteenth century, contemporary America requires, by Tocqueville's standards, a science of politics to combat the potential hazards of the easy ideas produced by literary writers about politics.

Over the past two decades students of American politics have re-discovered the concept of political culture, using it to investigate areas of society that for a time had been considered outside the normal boundaries of political science. Despite many recent advances in the techniques for studying social phenomena, modern researchers may still find that Tocqueville's approach offers some important insights. Whether or not one accepts his view about the relationship between basic "mental habits" and political values, it at least provides criteria for determining what is worthwhile and thus suggests a way of avoiding the sorts of inquiries that catalog every societal factor imagined to have a bearing on political beliefs. Moreover, far from merely making ritualistic claims about the centrality of political culture, Tocqueville shows how one can keep the concept continually in mind to analyze and evaluate institutions and public policies. Finally, Tocqueville's analysis of the connection between political thought and political culture—and between changes in the one and transformations in the other—points to the need to transcend the artificial modern division between political theory and empirical political science. If one follows Tocqueville's approach, political culture must be understood ultimately by reference to the philosophic ideas that form the mental structures which govern how people see the world and formulate their opinions. The task of maintaining a political culture of liberty leads to a consideration of the philosophic ideas capable of being embodied in the mental habits of citizens which can provide people with the means and the will to maintain their freedom.

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# Assessment of Political Power in the Israeli Knesset

AMNON RAPOPORT
*University of Haifa*

ESTHER GOLAN
*University of Tel Aviv*

*Immediately after the election to the tenth Israeli parliament (Knesset), 21 students of political science, 24 Knesset members, and seven parliamentary correspondents were each asked (a) to assess the political power ratios of the 10 parties represented in the Knesset and (b) to judge the ideological similarity between them. As ascertained by Saaty's analytic hierarchy scaling technique, the power ratio judgments proved sufficiently consistent to justify the construction of individual ratio scales of perceived political power. The ideological proximities were adequately represented by two-dimensional ideological spaces. Analyses of the derived power measures showed that the higher the political sophistication of the subject, the higher the combined power attributed to the religious parties and the lower the combined power assigned to the two largest parties* Likud *and* Labor. *The derived power measures were then compared to the predictions of six power indices, three of which only consider the ideological space. Of the six models, the generalized Banzhaf power index best accounted for the perceived power of 62% of the subjects, whereas the classical Shapley-Shubik index provided the best fit for 31% of the subjects. The generalized power indices were found only partly satisfactory with a need for further revision.*

There is general agreement among social scientists that the study of power is central to the understanding of conflicts and their resolution. This agreement encompasses conflict situations ranging from dyadic interactions to the formation and dissolution of conflicts in large weighted voting bodies. Thibaut and Kelley (1959) placed power in the center of their psychological investigation of the intricate and often subtle bargaining behavior in mixed-motive dyadic conflicts. Examining large social bodies, Parsons (1963) suggested that power is one of the key concepts in the Western tradition of thought about political phenomena, and Lasswell and Kaplan stated more than 30 years ago that "the concept of power is perhaps the most fundamental in the whole political science; the political process is the shaping, distribution, and exercise of power" (1950, p. 75). Yet despite this general consensus, there have been no attempts to study how people actually assess power in political voting bodies. Because the perception of political power in parliamentary democracies and in other political voting bodies may affect bargaining and influence policy decisions, the outcomes of such an empirical study, besides shedding light on an important aspect of human behavior, are potentially useful for answering questions about the formation of coalitions and the allocation of cabinet ministries among coalition parties.

In discussing the conceptual intractability of the concept of power, Parsons observed that "there is, on analytical levels, a notable lack of agreement both about its specific definition, and about many features of the conceptual context in which it should be placed" (1963, p. 235). Increasingly, Riker's (1962) cutting of the gordian knot of the notion of power, which replaces it by the more concrete notion of winning, has come to be adopted in the special and restricted domain of the distribution of power in weighted voting bodies. In correspondence with Riker's view, political scientists and other researchers have developed numerical indices of power for social systems that can be adequately modeled as simple games (Shapley, 1962), or cooperative $n$-person enterprises in which "winning" is the only goal and a specification of which coalitions are capable of doing so is the only structure.

Although numerical power indices have gained much popularity, our contention is that in their present form they are too limited for modeling

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Reprint requests should be addressed to Amnon Rapoport, Department of Psychology, University of Haifa, Mt. Carmel, Haifa 31999, Israel.

perceived voting power. Theoretical developments in the related and more familiar area of coalition formation, which started about a dozen years ago, may prove useful for descriptive purposes and point the way for constructing more adequate models. As is well known, Riker's seminal work on the size principle (1962) inspired the construction of models that proposed alternative criteria for the formation of coalitions in weighted majority games such as the formation of minimum winning coalitions (Riker, 1962), coalitions of minimum size (Gamson, 1961), and coalitions with a minimum number of members. Like the classical power indices of Shapley and Shubik (1954) and Banzhaf (1965), these criteria have all presupposed only knowledge of the distribution of weights (e.g., seat distributions in parliamentary systems). About a dozen years ago these models were successfully challenged by coalition theories which, in addition to the distribution of weights, incorporate notions like ideological space or party distance and postulate the minimization of some form of ideological diversity of the coalition (Axelrod, 1970; DeSwaan, 1973). For example, in his "conflict of interest" theory, Axelrod (1970) predicted the formation of "minimal connected winning coalitions." Connected coalitions consist of coalitions of parties that are adjacent in a one-dimensional ideological ordering and, hence, minimize the "conflict of interest." Minimal connected winning coalitions are connected coalitions for which the deletion of either extreme member renders the coalition losing. Extensions to multidimensional ideological spaces, which remove the constraint imposed by Axelrod, have been proposed more recently by Winer (1979) and Grofman (1982).

Empirical studies (DeSwaan, 1973; Taylor & Laver, 1973) have shown that coalition-formation theories that incorporate information about ideological proximity are descriptively more adequate than theories that disregard this information. The major purpose of the present study is to test the hypothesis that this same conclusion also holds in the area of perceived voting power. Specifically, we use an experimental approach to measure perceived voting power in the Israeli Knesset in order to test competitively six models of perceived voting power. Three of these six models are the power indices of Shapley and Shubik (1954), Banzhaf (1965), and Deegan and Packel (1979), which in general yield different predictions. The other three models generalize the first three power indices by accounting for both the party seat distribution and the ideological proximities between the political parties in the Knesset.

Two additional questions are addressed in an attempt to assess the validity and generality of our conclusons. The first issue is methodological. We attempt to ascertain how—if at all—voting power is perceived at a time when a political coalition is about to be formed. To measure perceived voting power, we use and test a recently proposed scaling procedure (Saaty, 1977, 1980) which possesses several desirable properties, among them an index of the consistency of the subject's responses and a ratio scale of perceived power. The ratio scale properties are required because the predictions of all six models are only unique up to a multiplication by a positive constant. The second question concerns the effects of political sophistication and experience of the respondents on their perception of power. To this end we have collected data from three groups of respondents, namely, graduate students of political science, Knesset members, and Knesset correspondents.

The second section defines several basic concepts of game theory and describes briefly the three power indices of Shapley and Shubik (1954), Banzhaf (1965), Deegan and Packel (1979), and their respective generalizations. The third section provides information about the subjects and the experimental procedure used to elicit their responses, and the remaining sections report the results of the study and discuss their implications.

## Power Indices for Simple Games

As mentioned above, measuring perceived voting power requires examining two sets of power indices, each set consisting of three indices. To understand these six indices and the predictions they make in our Knesset study, several basic concepts from game theory must first be reviewed.

### Simple Games

*n*-person cooperative games that model weighted voting bodies in which each coalition that might form can either "win" or "lose" are called simple (von Neumann & Morgenstern, 1947). Simple games are attractive because they constitute appropriate models for many *n*-person conflict situations, particularly in political structures such as parliaments and committees, but they are also appealing because the sparseness of their structure enables in-depth analyses of the game.

Consider a set $N = \{1, \ldots, n\}$ of $n$ players. Let $N$ be the power set of $\underset{\sim}{N}$ (the set of all $2^n$ possible coalitions), and define a partition of $\underset{\sim}{N}$ into two mutually exclusive and exhaustive sets: $\underset{\sim}{W}$, the set of all *winning* coalitions; and $\underset{\sim}{L}$, the set of all *losing* coalitions. Meaning is assigned to "winning" and "losing" by three requirements for this partition:

1) $\emptyset \in \underset{\sim}{L}$,
2) $N \in \underset{\sim}{W}$,
3) if $R \in \underset{\sim}{W}$ and $R \subset S$, then $S \in \underset{\sim}{W}$.

Conditions (1) and (2) guarantee that the null coalition of no players cannot win and that the grand coalition of all players must win, respectively. The heart of the definition is condition (3), which expresses the intuitive electoral meaning of "winning" in that no losing coalition may contain a winning coalition. A *simple game* can then be defined as a pair $(N, W)$, where $N$ is the set of players and $W$ is the set of all winning coalitions.

A further, almost universally applied, restriction is the condition

4) If $S \in \underset{\sim}{W}$, then $(N - S) \in \underset{\sim}{L}$.

Condition (4) guarantees that at most one coalition extracted at any single time from the body of $n$ players can be winning. Simple games satisfying condition (4) as well as conditions (1) through (3) are called *proper* (Shapley, 1962).

There are two types of player in simple games, which deserve mentioning. Player $i \in N$ is called a *dictator* if every coalition of which he or she is a member wins, and every coalition he or she is not in loses. A *dummy* is a player in a simple game who can never, by joining a losing coalition, change it to a winning one.

Interest in proper simple games most often centers on a subset of the winning coalitions called minimal winning coalitions, and denoted by $\underset{\sim}{W}^m$. A coalition $S$ is said to be *minimal winning* if every proper subset of $S$ is losing, whereas $S$ itself is winning.

A proper simple game can be completely specified by its set of minimal winning coalitions. Alternatively, it is sometimes possible to specify the set of winning coalitions by assigning non-negative integer "weights" (e.g., delegation votes and parliamentary seats) $w_1, w_2, \ldots, w_n$ to the players, and then specifying a criterion $W$ such that

$$S \in \underset{\sim}{W} \leftrightarrow \sum_{i \in S} w_i \geqslant W.$$

Games in which this specification is possible are termed *weighted majority games*; often, but not necessarily, $W$ is the integer just greater than $(w_1 + w_2 + \ldots + w_n)/2$. It is convenient to use a shorthand notation $[W; w_1, w_2, \ldots, w_n]$ to describe weighted majority games. This notation is known as a *weighted majority representation* of a simple game. A simple game may have more than one weighted majority representation. But when the weights have natural interpretations, as in parliamentary seats, there can be little quarrel about their nature.

**The Shapley-Shubik Power Index.** The Shapley-Shubik (SS) power index is a specialization of the Shapley (1953) value, which measures a player's power in terms of the relative expected frequency with which he or she turns a losing coalition into a winning coalition by casting the deciding vote. Consider all possible orderings of the $n$ players in a simple game, which may be taken as all of the potential ways of building up toward the grand coalition. For each of these $n!$ permutations, some unique player joins and thereby turnswhat was a losing coalition into a winning one; this unique player is called the *pivot*. The SS power index of any player $i$, denoted by $\phi_i$, is defined as the number of permutations in which player $i$ is the pivot divided by the total number of alignments. The SS index (Shapley & Shubik, 1954) is computed from

$$\phi_i = \Sigma[(s-1)!(n-s)!]/n!,$$

where $s = |S|$ is the number of players in coalition $S$, and the summation is taken over all winning coalitions $S$ for which coalition $S - \{i\}$ is losing. The power index for a simple game will be stated as the vector of power indices of its players, $(\phi_1, \phi_2, \ldots, \phi_n)$.

Although the pivotal-player interpretation leading to the SS index is sufficient grounds for entertaining it, interest is increased because it may be uniquely derived from an axiomatic characterization (Dubey, 1975) and may be given additional interpretations (Roth, 1977; Straffin, 1978).

**The Banzhaf Power Index.** Banzhaf (1965) introduced another index for measuring power in simple games, in which all combinations of voters are considered in constructing the index, rather than all permutations as with the SS index. The Banzhaf (BZ) index of power may be developed in much the same manner as the SS index. For a simple game $(N, \underset{\sim}{W})$ define a *swing* for player $i$ to be a coalition $S$, where player $i$ is a member of coalition $S$, $S$ is winning, and coalition $(S - \{i\})$ is losing. Let $\eta_i$ denote the number of swings for player $i$; $\eta_i$, the "raw" BZ index, counts the number of times player $i$ could change a coalition from losing to winning. Define

$$\beta_i' = \eta_i / 2^{n-1},$$

where $2^{n-1}$ is the total number of coalitions containing player *i*, and therefore the maximum number of swings player *i* could possibly have. The vector $(\beta'_1, \dots, \beta'_n)$ is called the *absolute BZ index*. In general, $\sum_{i=1}^{n} \beta'_i \neq 1$. However, as stated by Banzhaf, it is the ratio of "raw" Banzhaf indices which is of importance. It is common, therefore, to use the *normalized BZ index* $(\beta_1, \dots, \beta_n)$ where

$$\beta_i = \beta'_i / \sum_{i=1}^{n} \beta'_i, \quad i \in N.$$

Like the SS index, the BZ index has also been given an axiomatic characterization (Dubey, 1975; Dubey & Shapley, 1979) and alternative interpretations (Roth, 1977; Straffin, 1978).

**The Deegan-Packel Power Index.** Unlike the SS and BZ indices, which are based on a priori notions of power as the ability to be the decisive voter in an abstract legislative body, the Deegan-Packel (DP) index is an attempt to characterize power based on the observed ways in which voters in such legislatures supposedly behave. Deegan and Packel (1979, p. 122) intend "to reflect what we consider to be realistic dimensions of participant behavior and spoils distribution in certain game type situations; notably the exercising of power through interpersonal interactions that may be characterized as being 'political' in nature with all participants in victorious coalitions sharing equally in the spoils."

Following Riker (1962), the model assumes that only minimal winning coalitions will emerge. Formally, given a simple game $(N, \underset{\sim}{W})$, define the collection of all minimal winning coalitions that contain player *i* by

$$\underset{\sim}{W}^m(i) = \{S \in W^m \mid i \in S\}.$$

The DP power index for player *i*, denoted by $\rho_i$, is defined by

$$\rho_i = \frac{1}{|\underset{\sim}{W}^m|} \sum_{S \in \underset{\sim}{W}^m(i)} 1/s, \qquad (1)$$

where $|\underset{\sim}{W}^m|$ is the total number of minimal winning coalitions in the game and $s = |S|$ is as before. The DP power index is then the vector of individual player power indices, $(\rho_1, \dots, \rho_n)$.

Three assumptions regarding the behavior of players in simple games give rise to this definition of power:

1. Only minimal winning coalitions will form.
2. Players in a minimal winning coalition will divide the coalition payoff equally.
3. Each minimal winning coalition has an equal probability of forming.

Deegan and Packel (1979) show that the power index $\rho_i$ in equation (1) is consistent with and determined by these assumptions. They have also given their power index an axiomatic characterization. The resulting axiom set is similar to that for the SS index, but admittedly (Deegan & Packel, 1979, p. 117) lacks the compelling universality of earlier sets.

*Example.* To illustrate the three power indices above, consider a parliamentary legislation with four parties *A, B, C,* and *D* controlling 30, 20, 10, and 40 seats, respectively. Suppose that to pass a piece of legislation a two-thirds majority is required. Then this system may be represented as a weighted majority game

$$[67; \overset{A}{30}, \overset{B}{20}, \overset{C}{10}, \overset{D}{40}]$$

This game has neither a dictator nor dummy players.

There are altogether 4! = 24 permutations of the four parties. Parties *A, B, C,* and *D* occupy pivotal positions in 6, 2, 2, and 14 permutations (orderings), respectively. Hence,

$$(\phi_A, \phi_B, \phi_C, \phi_D) = \frac{1}{24}(6, 2, 2, 14)$$
$$= (0.250, 0.083, 0.083, 0.583).$$

To compute the Banzhaf values, consider all the winning coalitions in the game: *AD, ABD, ACD, BCD,* and *ABCD*. Counting the number of swings yields $\eta_A = 3, \eta_B = \eta_C = 1$, and $\eta_D = 5$. Therefore,

$$(\beta_A, \beta_B, \beta_C, \beta_D) = \frac{1}{10}(3, 1, 1, 5)$$
$$= (0.333, 0.100, 0.100, 0.500).$$

To compute the DP power values, note that there are two minimal winning coalitions in the game: $\underset{\sim}{W}^m = \{AD, BCD\}$. Equation (1) yields directly

$$\rho_A = 1/2(1/2) = 1/4, \rho_B = \rho_C = 1/2(1/3)$$
$$= 1/6, \rho_D = 1/2(1/2 + 1/3) = 5/12.$$

The DP values are given by:

$$(\rho_A, \rho_B, \rho_C, \rho_D) = \frac{1}{12}(3, 2, 2, 5)$$
$$= (0.250, 0.167, 0.167, 0.417).$$

In the present example the DP index provides the most egalitarian results, the SS index gives the least egalitarian results, and the BZ index falls in between. Each of the three power indices assigns the same voting power to parties *B* and *C* despite the 10 seat difference between them.

### Nonsymmetric Generalizations of Power Indices

Even a cursory examination of the three power indices shows that the analysis of political voting situations they provide is in many ways insufficient or superficial. The power indices described above ignore the realities of political, economic, or psychological life where alliances, cohesive blocs, discrimination, favoritism, and partisan actions are the rule rather than the exception. Although it is possible that some or perhaps most of these factors will play only insignificant roles in the perception of voting power, it seems unlikely that all of them will be ignored. Experience suggests that some voting combinations or coalitions are more likely to form than others. Notwithstanding individual differences in weights, this asymmetry is most likely to be caused by ideological proximity in parliamentary systems.

Recognizing the seriousness of the limitation of the power indices described above, Owen (1971) proposed a generalization of the Shapley (1953) value, which explicitly considers the affinity among players. In a subsequent article, Shapley (1977) generalized the SS power index to account for asymmetry in preferences, habits, or outlooks of the players. Shenoy (1980) later proposed a similar generalization of the BZ index. And the DP index, too, may be generalized in a straightforward manner. All three generalizations are computational schemes that consider both the weight distribution and ideological proximity between all pairs of players. In all three generalizations, the $n$ players are assumed to be represented as $n$ points in some politically interpretable multidimensional Euclidean space, and the ideological proximity is assumed to be proportional to the Euclidean distance in this space.

**The Generalized Shapley-Shubik Power Index.** To allow for proximities between players, Shapley (1977) proposes representing the players as $n$ points in an $m$-dimensional Euclidean space, $R^m$. This representation is quite common in psychological studies of cognition and nonmetric multidimensional representations of similarity data (Kruskal & Wish, 1978). It leaves ample scope for capturing many kinds of parameters without an excess of arbitration, including the intuitive ideas of "modernization" and "extremism" which are prominent in political analyses (Shapley, 1977).

Both political issues and profiles are represented in the generalized model by $m$-dimensional vectors regarded as belonging to different, dual spaces. Figure 1 illustrates this representation for $n = 5$ and $m = 2$. The two arrows indicate the directions associated with two typical issues. Figure 1 shows that each arrow determines an order of the five players by the simple process of dropping perpendiculars to the shafts of the arrows. As an arrow turns, different player-orders are produced; a 180° turn reverses the order. Turning an arrow 360° may not generate all $n!$ possible orders. For example, in Figure 1 player *B* will never appear in any order as the initial or final element. When $m$ is small and $n$ is relatively large, as will be the case in many applications, the model envisages that only a very small fraction of the possible alignments of players will actually be ideologically consistent.

It is assumed that all issue directions are equally likely—"that the 'political winds' blow across the ideological space in a perfectly random way" (Shapley, 1977, p. 20). An arrow $\xi$ can be chosen that represents an issue according to the uniform probability distribution over the unit sphere defined by

$$S^m = \{\,\xi \in R^m \mid (\xi_1^2 + \xi_2^2 + \ldots + \xi_m^2)^{1/2} = 1\}. \quad (2)$$

Then the probability that $\xi$ lies in any given region in $S^m$ is proportional to the area of that region.

Now, for each player $i \in N$ and each issue (arrow) $\xi$ define $P_i(\xi)$ as the set of players who are more enthusiastic about the issue direction $\xi$ than $i$ is. By "more enthusiastic" is meant that these players are ranked below player $i$ in the player order generated by $\xi$. Also define $S_i^m$ to be the set of issue directions in $S^m$ for which $P_i(\xi) \in \underset{\sim}{L}$ and $P_i(\xi) \cup \{\,i\,\} \in \underset{\sim}{W}$. In other words, $S_i^m$ is the set of issue directions for which player $i$ serves as pivot in the simple game. Then the generalized Shapley-Shubik (GSS) power index is given by

$$\phi_i^g = \frac{\text{area of } S_i^m}{\text{area of } S^m}, \text{ for every } i \in N.$$

As expected, $\sum_{i \in N} \phi_i^g = 1$. When $m = n$ and the $n$ points are the unit vectors of $R^m$, GSS reduces to SS. Examples show that the GSS model has the desirable property that if player $i$

occupies a "central" position in $R^m$ his or her power is enhanced relative to the SS model.

**The Generalized Banzhaf Power Index.** Shenoy (1980) proposed a generalization of the BZ power index, which combines elements of Shapley's generalization of the SS index and Straffin's (1978) probabilistic interpretation of both the SS and BZ indices. Shenoy's nonsymmetric generalization may be described as a sequence of three steps.

The first step assumes that the players are presented as $n$ points in $R^m$. Each dimension of $R^m$ is interpretable in terms of psychological, economic, or political parameters, exactly as in the traditional factor analysis and nonmetric multidimensional scaling techniques. The ideological space is a subspace of $R^m$ defined by

$$B^m_{1/2} = \{ \mathbf{x}^i \in R^m | ((x^i_1)^2 + \ldots + (x^i_m)^2)^{1/2} \leqslant 1/2 \}, \quad i \in N.$$

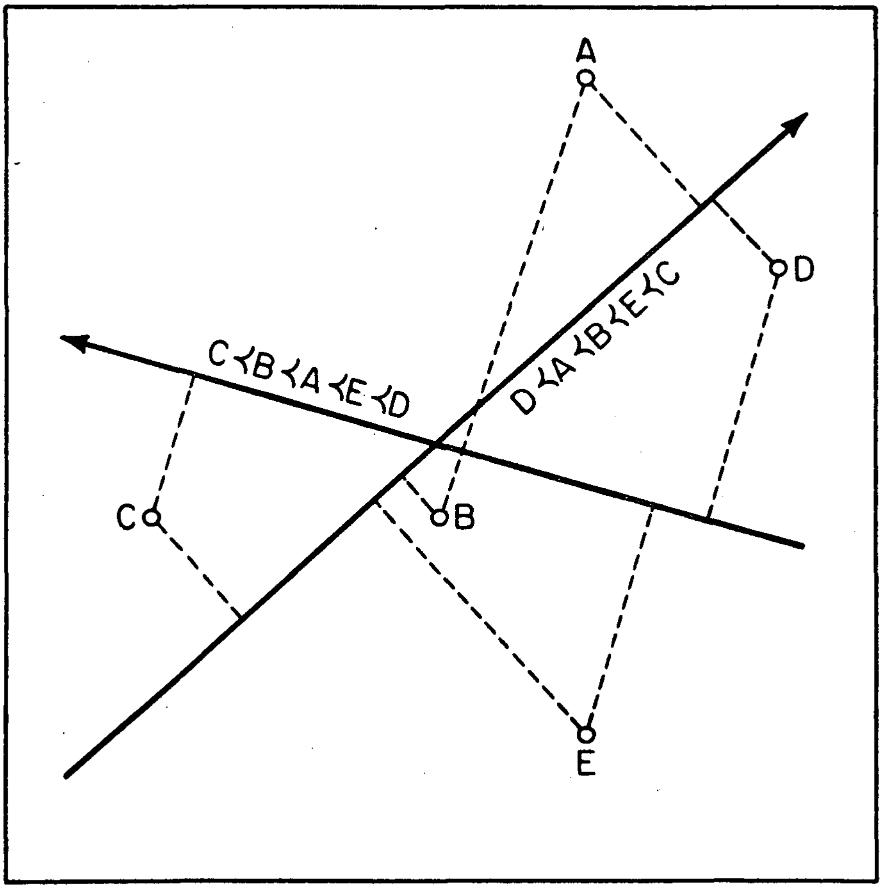
Each player $i$ is thus represented by a point $\mathbf{x}^i$ in $B^m_{1/2}$, which is called the profile of player $i$.

To implement the second step of Shenoy's computational scheme, it is assumed that the assembly of players will be called to vote on a series of proposals or bills called *issues*. An issue in the model is a linear function on $R^m$ represented by a point $\xi$ on the unit sphere $S^m$ (Eq. 2). An issue, then, is some combination of the parameters of the ideological space. Although the players' profiles $\mathbf{x}^i$ and the issues $\xi$ are both $m$-dimensional, like in the Shapley (1977) generalization, they should be regarded as belonging to different, dual spaces, not the same space.

Let $p^i_\xi$ denote the probability that player $i$ will vote "yes" for issue $\xi$, where

$$p^i_\xi = \xi_1 x^i_1 + \xi_2 x^i_2 + \ldots + \xi_m x^i_m + 1/2. \tag{3}$$

**Figure 1. A Two-Dimensional Representation of Five Players with Two Different Issue Directions**



Equation (3) shows that the probability that player $i$ will support issue $\xi$ is proportional to his or her distance from the issue direction $\xi$. $p^i_\xi$ satisfies $0 \leqslant p^i_\xi \leqslant 1$ for every $\mathbf{x}^i \in B^m_{1/2}$ and every $\xi \in S^m$ and hence is a well-defined probability.

In the third step of the computational scheme the generalized Banzhaf (GBZ) power index is computed. Consider first some issue $\xi$. Then the swing probability for player $i$, $\Pi^i_\xi(p)$, is the probability that an issue $\xi$ that player $i$ supports passes but would fail were $i$ to change his or her vote, or that the issue $\xi$ that player $i$ opposes fails but would pass if player $i$ changes his or her vote.

The GBZ power index for player $i$, denoted by $\beta^g_i$, is the probability that player $i$ will make a difference in the outcome summed over all issues. To compute $\beta^g_i$, Shenoy makes the same simplifying assumption as Shapley, namely, that all issue directions in the unit sphere are equally likely. If the model is assigned a political interpretation with players corresponding to political parties and issues to legislative proposals, this latter assumption amounts to requiring that all types of bills are equally likely to be introduced to the multi-party parliament.

**The Generalized Deegan-Packel Power Index.** A nonsymmetric generalization of the DP power index, which utilizes information about the ideological proximity between the players, may take one of several forms. To allow comparison with the two generalized indices GSS and GBZ, the approach that we propose also represents the players as $n$ points in $R^m$ Assumptions (1) and (2) of the original DP model are retained, but the third, and to our mind more controversial, assumption of equiprobability of minimal winning coalitions is replaced by the assumption that the probability of forming a minimal winning coalition is inversely proportional to the mean of the Euclidean distances between all members of this coalition. Thus, the more cohesive is the minimal winning coalition in the political space, the more likely it is to form.

Formally, let $p(S)$ denote the probability that some minimal winning coalition $S \in \underset{\sim}{W}^m$ will form. Whereas in the original DP model $p(S) = 1/|\underset{\sim}{W}^m|$, in the present model $p(S)$ depends on the inter-player distances $d(i,j)$, where

$$d(i,j) = \Big(\sum_{k=1}^{m} (x^i_k - x^j_k)^2\Big)^{1/2}, \quad \text{for } i,j \in S.$$

Denote the mean distance for coalition $S$ by

$$d(S) = \sum_{i,j \in S} d(i,j)/h, \quad \text{for } i < j,$$

where $h$ is the number of pairs of players in the coalition $S$. Then the non-normalized probability that coalition $S$ will form is given by $p'(S) = 1/d(S)$, which after normalization becomes

$$p(S) = p'(S) / \sum_{S \in \underset{\sim}{W}^m} p'(S).$$

Finally, the generalized Deegan-Packel (GDP) power index for player $i$, denoted by $\rho^g_i$, is defined by

$$\rho^g_i = \sum_{S \in \underset{\sim}{W}^m(i)} p(S)/s, \quad \text{for } i \in N.$$

**Example.** To illustrate the effect of ideological centrality on the three generalized power indices and to compare them to each other, consider a political voting body with three parties $A$, $B$, and $C$ controlling 20, 10, and 10 votes, respectively. Assuming a regular majority for passing legislation, this voting body can be represented by the weighted majority game [21;20,10,10]. Party $A$ is a veto player in this game; no coalition may form without it.

Suppose that in terms of their ideological positions the three parties are represented in a 2-dimensional ideological space $B^2_{1/2}$ such that party $A$ is in the center of this space and parties $B$ and $C$ are located on its edge. Specifically, assume that the profiles of the three parties satisfy $x^A_1 = x^A_2 = 0$, $(x^B_1)^2 + (x^B_2)^2 = (1/2)^2$, and $(x^C_1)^2 + (x^C_2)^2 = (1/2)^2$. This assumption implies that $A$, $B$, and $C$ form an isosceles triangle in which $d(A,B) = d(A,C) = 1/2$, whereas the distance $d(B,C)$ may vary from zero to one depending on the angle $\measuredangle BAC$, which will be denoted by $\alpha$ $(0° \leqslant \alpha \leqslant 180°)$. If $\alpha = 0°$, parties $B$ and $C$ occupy the same position in the ideological space, whereas if $\alpha = 180°$, parties $C$, $A$, and $B$ form a single ideological dimension with $d(B,C) = 1$. Hence, $\alpha$ measures the centrality of $A$ with respect to the ideological positions of $B$ and $C$.

The upper part of Table 1 presents the GSS, GBZ, and GDP values for selected values of $\alpha$. Consider first the GSS power index. Table 1 shows that for the present example $\phi^g_A$ increases linearly in $\alpha$, assuming the value 0.5 when $\alpha = 0°$ and 1 when $\alpha = 180°$. When $\alpha = 60°$, $A$, $B$, and $C$ form an equilateral triangle. In this case,

$$(\phi^g_A, \phi^g_B, \phi^g_C) = (\phi_A, \phi_B, \phi_C) = (2/3, 1/6, 1/6).$$

Consider next the GBZ index. As before, the voting power of party $A$ is enhanced as the

**Table 1. Generalized Power Indices for Two 3-Person Weighted Majority Games**

| | GSS | | | GBZ | | | GDP | | |
|---|---|---|---|---|---|---|---|---|---|
| $\alpha$ | $\phi^g_A$ | $\phi^g_B$ | $\phi^g_C$ | $\beta^g_A$ | $\beta^g_B$ | $\beta^g_C$ | $\rho^g_A$ | $\rho^g_B$ | $\rho^g_C$ |
| Game: $[21; \overset{A}{20},\overset{B}{10},\overset{C}{10}]$ | | | | | | | | | |
| 0° | 0.500 | 0.250 | 0.250 | 0.555 | 0.222 | 0.222 | 0.500 | 0.250 | 0.250 |
| 30° | 0.583 | 0.208 | 0.208 | 0.562 | 0.219 | 0.219 | 0.500 | 0.250 | 0.250 |
| 60° | 0.667 | 0.167 | 0.167 | 0.579 | 0.211 | 0.211 | 0.500 | 0.250 | 0.250 |
| 90° | 0.750 | 0.125 | 0.125 | 0.600 | 0.200 | 0.200 | 0.500 | 0.250 | 0.250 |
| 120° | 0.833 | 0.083 | 0.083 | 0.619 | 0.190 | 0.190 | 0.500 | 0.250 | 0.250 |
| 150° | 0.917 | 0.042 | 0.042 | 0.632 | 0.184 | 0.184 | 0.500 | 0.250 | 0.250 |
| 180° | 1.000 | 0 | 0 | 0.636 | 0.182 | 0.182 | 0.500 | 0.250 | 0.250 |
| Game: $[21; \overset{A}{10},\overset{B}{20},\overset{C}{10}]$ | | | | | | | | | |
| 0° | 0 | 0.750 | 0.250 | 0.111 | 0.667 | 0.222 | 0 | 0.500 | 0.500 |
| 30° | 0.083 | 0.709 | 0.208 | 0.124 | 0.657 | 0.219 | 0.171 | 0.500 | 0.329 |
| 60° | 0.167 | 0.667 | 0.167 | 0.158 | 0.632 | 0.211 | 0.250 | 0.500 | 0.250 |
| 90° | 0.250 | 0.625 | 0.125 | 0.200 | 0.600 | 0.200 | 0.293 | 0.500 | 0.207 |
| 120° | 0.333 | 0.584 | 0.083 | 0.238 | 0.571 | 0.190 | 0.317 | 0.500 | 0.183 |
| 150° | 0.417 | 0.541 | 0.042 | 0.264 | 0.552 | 0.184 | 0.330 | 0.500 | 0.170 |
| 180° | 0.500 | 0.500 | 0 | 0.273 | 0.545 | 0.182 | 0.333 | 0.500 | 0.167 |

centrality of *A* increases. However, the effect of centrality on $\beta^g_A$ is weaker than its effect on $\phi^g_A$: $\beta^g_A$ varies from 5/9 for $\alpha = 0°$ to 7/11 for $\alpha = 180°$. When $\alpha = 90°$, the GBZ and BZ values coincide for the present example.

The degree of centrality of player *A* has no effect on the GDP values for the game [21; 20,10,10] because the GDP power index, as well as the DP index, only considers minimal winning coalitions in assessing power. In the present example there are only two minimal winning coalitions, *AB* and *AC*, and the distance between their members is unaffected by varying the ideological positions of parties *B* and *C*.

To construct the lower part of Table 1, we exchanged the number of votes controlled by parties *A* and *B* without changing the parties' profiles. The result is a new game [21; 10,20,10] in which *A* maintains its ideological centrality though not its veto power. Parties *A* and *C* are symmetrical in terms of the number of votes they control but not in their ideological profiles. Table 1 shows that for all three generalized power indices, the power of the central party *A* is enhanced as its centrality increases. The strongest effect of ideological centrality is obtained for the GSS index and the weakest for the GBZ index. Because the set of minimal winning coalitions now includes coalition *BC* as well as *AB*, the centrality of *A* affects the GDP as well as the GSS and GBZ values.

The two example games above, as well as other examples not reported here, show that for weighted majority games of the kind discussed in the present article, the three generalized power indices are all sensitive to the ideological profiles of the players, albeit in different degrees. They generally enhance the power of the ideologically more central parties and reduce the power of the ideologically more extreme parties.

## Method

### The Tenth Knesset

The institutional framework of the Israeli political system was largely established by the provisional government, founded on April 12, 1948, by the Zionist General Council. The Provisional Council chose universal suffrage and a simple proportional representation system, which treats the entire nation as a single constituency for the 120-member *Knesset* (parliament). The task of forming a government is entrusted by the president to a member of the Knesset, typically but not necessarily the leader of the party commanding the largest number of seats. The composition of the government is a topic for party negotiations; it is only partially defined as consisting of a prime minister and an unspecified number of ministers, who may or may not be parliament members. Once a government is formed, it should immediately present itself to the Knesset and be regarded as constituted when it has received from the Knesset its vote of confidence.

**Table 2. Frequency Distributions of Party Membership for the Tenth Knesset and for Group K**

| | Knesset | | Group K | |
|---|---|---|---|---|
| Party | Frequency | Proportion | Frequency | Proportion |
| DFP | 4 | 0.033 | 1 | 0.042 |
| DMC | 2 | 0.017 | 1 | 0.042 |
| CRM | 1 | 0.008 | 0 | 0 |
| LP | 47 | 0.392 | 9 | 0.375 |
| MNR | 2 | 0.017 | 1 | 0.042 |
| U | 48 | 0.400 | 11 | 0.458 |
| MIT | 3 | 0.025 | 0 | 0 |
| NRP | 6 | 0.050 | 1 | 0.042 |
| AI | 4 | 0.033 | 0 | 0 |
| R | 3 | 0.025 | 0 | 0 |
| Total | 120 | 1.000 | 24 | 1.000 |

The election to the tenth Knesset took place on July 1981 after a long and bitter campaign. Table 2 lists the ten parties that elected members to the Knesset together with the seat distribution. For a detailed description of the Israeli political system, see Arian (1966, 1973) and Seliktar (1982). Here we only present a brief characterization of the ten parties.

*Hadash,* which is an acronym for *Hazit Democratit Leshalom* (Democratic Front for Peace, or DFP), is an orthodox, pro-Russian, anti-Zionist Communist party, committed to an aggressive class struggle and identified with the Arab stand in the Middle East conflict.

*Hatnua Hademocratit Leshinui* (Democratic Movement for Change, or DMC) favors liberal economic and welfare policies and a moderate approach to the Israeli-Arab conflict.

*Hatnua Lezhuiot Haezrah* (the Civil Rights Movement, or CRM) is primarily interested in protecting the secular character of the State of Israel and the civil rights of its citizens. The CRM advocates a liberal economic policy and a dovish stand in the Israeli-Arab conflict.

The *Labor Party* (LP) was established in 1968 by a merger of three parties—*Mapai, Ahdut-ha-Avoda,* and *Rafi.* The LP is the most pluralistic party in Israeli politics. Undergoing an evolutionary development since 1948 by perceptibly shifting its position from a pronounced left-wing ideology to a more left-of-center position, the LP professed in 1981 a moderate socialistic and welfare-state orientation toward socioeconomic issues without negating the role of a capitalistic economy. It accepted a modern degree of nonseparation between state and religion. Adopting a pro-Western orientation in foreign affairs, it steered a middle course between the maximalist and minimalist demands by evolving a program that seeks a peace settlement with the Arabs while maintaining control of part of the occupied territories (Seliktar, 1982).

*Hatnua Lehithadshut Mamlactit* (Movement for National Revival, or MNR) espouses the superiority of the state mechanism over its sectional components like the *Histadrut*—the general trade union organization in Israel. It opposes the imposition of a rigid wage structure by the *Histadrut* and its protection of labor rights to the detriment of efficient management, and it advocates a right-to-center stand on the Arab-Israeli conflict.

*Likud* (Union, or U) is a bloc of right-wing parties led by *Herut,* which for 30 years has been the main opposition party. Reflecting conservative principles, *Likud* calls for a laissez-faire economic policy and the abolition of *Histadrut* controls. Adhering to a highly nationalistic foreign policy, most of the party's activity has been directed toward promoting its national policy, which includes territorial demands and a militant stand toward the Palestinian issue. The *Likud* has maintained control of the Knesset from 1977.

*Tenuat Masoret Israel* (Movement for Israeli Tradition, or MIT) is a religious party primarily interested in improving the socioeconomic conditions and enhancing the political power of Oriental Jews.

*Miflaga Datit Leumit* (National Religious Party, or NRP) is primarily concerned with incorporating religious laws into the law of the state, especially in education, with defining the Jewish status of individuals, and with family laws. The party is committed to a liberal economy; it promotes a nationalistic foreign policy which includes territorial demands, although to a less extent than *Likud.*

*Agudat Israel* (Association of Israel, or AI) is an extremely religious party, rigidly committed to the rule of the Jewish religion. It is an anti-establishment party that, though participating in secular coalitions highly incompatible with its stand, has endeavored to impose its philosophy through an extreme opposition.

*Hathia* (Renaissance, or R) is an ultranationalist right-wing party, which left the *Likud* because of its maximal territorial demands and extremely nationalistic foreign policy.

## Subjects

Three groups of subjects participated in the study. Group *S* includes 21 undergraduate and graduate students of political science at the University of Haifa, who had to satisfy three requirements: They had taken at least one course on the structure of the Israeli political system, they were present on campus on the day the study was conducted (during the final examination period), and they agreed to participate in the study before taking their final examination.

Group *K* contains 24 Knesset members who were selected as follows. Immediately after the official announcement of the outcomes of the election to the tenth Knesset, letters were hand delivered to the 120 newly elected members, asking them to participate in a study on the assessment of political power and ideological similarity between parties. The Knesset members were each asked to devote approximately 90 minutes to an individual interview to take place at the time and location of his or her choosing. No response to the letter was required. One to two days later attempts were made to contact the Knesset member and secure the desired interview, which could only take place between the fifth and eighth day after the elections. Several Knesset members could not be located or approached, many refused to be interviewed for personal or political reasons, and others would not or could not work the interview into their schedule, typically mentioning the very short notice as the reason for their refusal.

Table 2 shows the distribution of the members of group *K* by party affiliation. The difference between the relative frequency distribution for group *K* and the population frequency distribution, shown on the left-hand side of Table 2, is statistically insignificant ($\chi^2(2) = 0.42$). Although the subjects of group *K* were not chosen randomly, there is no reason to doubt their representativeness.

Group *R* includes seven senior parliamentary correspondents of the Israeli media, who have been covering parliamentary events in the Knesset for some time. Group *R* consists of all the parliamentary reporters who agreed to the interview and succeeded in working it into their very tight schedule.

## Experimental Procedure

Ideological spaces and party distance matrices have been typically constructed from party voting records or from proximity data collected while a particular government was in power. The ideological positions obtained by either of these methods are not entirely suitable for use in any attempt to assess perceived voting power because the parties' voting power is itself partially determined by the membership of the government (Taylor & Laver, 1973). To overcome this methodological difficulty, the timing of the study was chosen with much care. By having subjects assess ratios of voting power and subsequently ideological similarity between pairs of parties after the election to the tenth Knesset but before the formation of a coalition government, we have ensured as much as possible that the membership of the government has no effect on the subjects' responses.

Within a brief period of four days, two practiced experimenters delivered the instructions, elaborated on them when necessary, and collected the data. Whereas the subjects of group *S* were tested collectively in a classroom, the subjects of groups *K* and *R* were tested individually either in their offices or in the Knesset building. For all three groups, the study lasted between 60 and 90 minutes.

The task consisted of two separate parts. The first part used the scaling technique of Saaty (1977, 1980) for measuring political power on a ratio scale (see the Appendix). Each subject was given written instructions which told him or her that the purpose of the study was to assess the political power of the parties in the tenth Knesset. Although the notion of power was not defined, three examples of a hypothetical Knesset with only four parties were presented in weighted majority form to illustrate its complexity.

After the written instructions and their verbal elaboration, the subject was given a response sheet and a list of the ten parties in the Knesset with the number of seats they control. Then all 45 pairs of parties were randomly presented to the subject, one at a time. The subject was asked to attend to each pair separately and to indicate on the response sheet 1) which of the two parties was politically more powerful, and 2) by how much. No constraints were placed on the response scale; all rational numbers equal or larger than 1 were permitted. No time constraint was imposed; the experiment was self-paced.

In the second part of the task, similarity judgments were collected to be later subjected to nonmetric multidimensional scaling (Coxon, 1982; Davison, 1983) for recovering the subject's political space. The task was again self-paced. All 45 pairs of parties were presented to the subject in a different random order, one at a time. For each pair the subject was asked to specify the degree of political or ideological similarity on a 0-100 response scale.

## Results

### Consistency of Power Ratio Judgments

The matrix **A** was constructed for each subject separately from the $n(n-1)/2$ power ratio judgments (see the Appendix), and then the maximal eigenvalue (denoted hereafter by $\lambda$ rather than $\lambda_{max}$) of **A** was extracted and the consistency index was computed from Eq. (A2). The values of $\lambda$ and $\mu$ are presented in Table 3, which also shows the response scales of 1 to $r$ used by the individual subjects.

Of the 52 subjects, 45 chose to use numbers (mostly integers but occasionally fractions) between 1 and 10, 5 used numbers between 1 and 9, one subject between 1 and 8, and another subject between 1 and 50. Because almost all the subjects restricted themselves to a 1-9 or 1-10 response scale, the Monte Carlo results reported by Saaty and mentioned in the Appendix are applicable to our data. Table 3 shows that none of the eigenvalues exceeded or even approached the critical level 19.66. The largest value of $\lambda$ in Table 3 is 14.16 (K20), and only 9 of the 52 $\lambda$'s exceed 12. Turning next to the consistency index $\mu$ (Eq. (A2)), Table 3 shows that for only 10 of the 52 subjects $\mu > 0.2$.

Combining the results of the two consistency tests, we concluded that for most of the subjects a meaningful ratio scale of political voting power could be constructed. Although the consistency of the responses of 10 subjects may be suspect, it was decided not to omit any subjects from subsequent analyses.

### Analysis of Perceived Power

Having established the consistency of the subjects' pairwise comparisons, the eigenvectors of **A** were next extracted, one for each subject. We shall refer to the eigenvector **w** ($w_i \geqslant 0$, $i=1,\ldots,n$; $w_1+\ldots+w_n=1$) as the vector of perceived power. Table 4 presents the means and standard deviations of the perceived power of the ten parties, computed separately for each group and over all 52 subjects.

A comparison of Tables 2 and 4 shows that the four mean vectors of perceived power differ from the vector of proportions of Knesset seats controlled by the parties. Perceived power in the present study cannot be solely accounted for in terms

**Table 3. Eigenvalues, Consistency Measures, Response Range, and Two Goodness-of-Fit Measures for Subjects of All Groups**

| Subject | $\lambda$ | $\mu$ | r | RSQ | STR | Subject | $\lambda$ | $\mu$ | r | RSQ | STR |
|---|---|---|---|---|---|---|---|---|---|---|---|
| S01 | 11.08 | 0.12 | 10 | 0.90 | 0.15 | K06 | 10.35 | 0.04 | 10 | 0.93 | 0.12 |
| S02 | 10.25 | 0.03 | 10 | 0.86 | 0.17 | K07 | 10.22 | 0.03 | 10 | 0.90 | 0.16 |
| S03 | 10.21 | 0.02 | 10 | 0.81 | 0.17 | K08 | 10.51 | 0.06 | 10 | 0.93 | 0.12 |
| S04 | 11.30 | 0.14 | 10 | 0.84 | 0.16 | K09 | 11.24 | 0.14 | 10 | 0.85 | 0.19 |
| S05 | 10.47 | 0.05 | 9 | 0.96 | 0.09 | K10 | 11.51 | 0.17 | 10 | 0.92 | 0.14 |
| S06 | 10.95 | 0.11 | 10 | 0.91 | 0.15 | K11 | 10.56 | 0.06 | 10 | 0.84 | 0.15 |
| S07 | 11.18 | 0.13 | 10 | 0.89 | 0.18 | K12 | 12.53 | 0.28 | 10 | 0.77 | 0.18 |
| S08 | 10.85 | 0.10 | 10 | 0.86 | 0.16 | K13 | 13.89 | 0.43 | 10 | 0.94 | 0.12 |
| S09 | 10.56 | 0.06 | 10 | 0.82 | 0.17 | K14 | 10.49 | 0.06 | 10 | 0.80 | 0.21 |
| S10 | 10.30 | 0.03 | 10 | 0.83 | 0.17 | K15 | 10.96 | 0.11 | 10 | 0.88 | 0.15 |
| S11 | 11.06 | 0.12 | 10 | 0.93 | 0.13 | K16 | 10.36 | 0.04 | 10 | 0.95 | 0.11 |
| S12 | 10.94 | 0.11 | 10 | 0.92 | 0.12 | K17 | 12.98 | 0.33 | 10 | 0.90 | 0.15 |
| S13 | 10.71 | 0.08 | 50 | 0.78 | 0.19 | K18 | 10.64 | 0.07 | 10 | 0.88 | 0.19 |
| S14 | 10.96 | 0.11 | 10 | 0.77 | 0.22 | K19 | 10.95 | 0.11 | 9 | 0.87 | 0.15 |
| S15 | 10.62 | 0.07 | 10 | 0.76 | 0.21 | K20 | 14.16 | 0.46 | 10 | 0.92 | 0.17 |
| S16 | 12.12 | 0.24 | 10 | 0.86 | 0.16 | K21 | 13.45 | 0.38 | 10 | 0.87 | 0.14 |
| S17 | 12.53 | 0.28 | 10 | 0.95 | 0.10 | K22 | 10.97 | 0.11 | 10 | 0.86 | 0.15 |
| S18 | 10.96 | 0.11 | 10 | 0.82 | 0.17 | K23 | 11.16 | 0.13 | 9 | 0.94 | 0.12 |
| S19 | 10.71 | 0.08 | 10 | 0.85 | 0.20 | K24 | 11.85 | 0.21 | 10 | 0.79 | 0.18 |
| S20 | 10.43 | 0.05 | 10 | 0.90 | 0.13 | R01 | 11.35 | 0.15 | 10 | 0.85 | 0.17 |
| S21 | 11.28 | 0.14 | 9 | 0.81 | 0.18 | R02 | 11.22 | 0.14 | 10 | 0.92 | 0.15 |
| K01 | 12.21 | 0.25 | 10 | 0.91 | 0.15 | R03 | 11.18 | 0.13 | 10 | 0.88 | 0.16 |
| K02 | 11.65 | 0.18 | 10 | 0.94 | 0.13 | R04 | 11.05 | 0.12 | 10 | 0.93 | 0.11 |
| K03 | 10.64 | 0.07 | 9 | 0.88 | 0.15 | R05 | 10.64 | 0.07 | 10 | 0.87 | 0.16 |
| K04 | 12.59 | 0.29 | 10 | 0.84 | 0.17 | R06 | 11.50 | 0.17 | 10 | 0.88 | 0.17 |
| K05 | 10.29 | 0.03 | 8 | 0.88 | 0.15 | R07 | 11.36 | 0.15 | 10 | 0.95 | 0.09 |

Table 4. Means and Standard Deviations of Perceived Voting Power

| | Group S | | Group K | | Group R | | Overall | |
|---|---|---|---|---|---|---|---|---|
| Party | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| DPF | .026 | .010 | .020 | .012 | .018 | .006 | .022 | .010 |
| DMC | .029 | .009 | .026 | .012 | .020 | .004 | .027 | .010 |
| CRM | .025 | .009 | .022 | .010 | .020 | .005 | .023 | .009 |
| LP | .187 | .091 | .162 | .082 | .109 | .080 | .165 | .087 |
| MNR | .044 | .017 | .045 | .016 | .038 | .016 | .044 | .015 |
| U | .338 | .059 | .323 | .091 | .292 | .081 | .325 | .065 |
| MIT | .060 | .024 | .072 | .031 | .104 | .031 | .071 | .030 |
| NRP | .139 | .045 | .149 | .042 | .175 | .055 | .149 | .043 |
| AI | .108 | .046 | .130 | .049 | .173 | .047 | .127 | .048 |
| R | .043 | .016 | .052 | .057 | .052 | .014 | .048 | .018 |

of the distribution of seats. The comparison shows that the mean perceived power of the Labor and Likud parties was considerably lower than their proportion of seats. On the other hand, the perceived power of each of the three religious parties MIT, NRP, and AI exceeded their proportion of seats by a factor of 3 or 4; the three religious parties marshal together only 10.8% of the Knesset seats, whereas their combined perceived power averaged over all the subjects equals 34.7%.

The discrepancy between objective power, as measured by the proportion of seats, and subjective or perceived power may be explained in terms of the likelihood of being included in a winning coalition and thereby sharing the spoils. Several observations of parties with the same number of seats but different perceived power support this explanation. DFP, which had no chance of entering a coalition, even a coalition of national unity, was assigned a mean power of 0.022, whereas AI, which had the same number of seats but was considered by most analysts as a highly likely candidate for membership in the coalition, was assigned a mean power approximately six times larger. Perhaps less dramatic but more important is the comparison between the two major parties U and LP, which controlled 48 and 47 seats, respectively. Probably sharing the general consensus that U was more likely to form a winning coalition than LP, a consensus that proved true later, the subjects in the present study judged on the average that U was twice as politically powerful as LP.

Table 4 also shows group differences in the mean perceived power of several parties. As the political sophistication of the group members grows, the mean perceived power of LP drops from 0.187 for group S through 0.162 for group K to 0.109 for group *R*. The mean perceived power of U also decreased in the same direction, although at a slower rate, from 0.338 for group S through 0.323 for group K to 0.292 for group R. Owing to large within-group variability, neither of these two group differences was statistically significant by a one-way ANOVA ($p \geqslant .05$). However, when the perceived powers of both dominant parties were combined, a one-way ANOVA yielded a significant group effect ($F(2,49) = 3.51$, $p < .05$). In a similar fashion, the powers assigned to MIT, NRP, and AI were combined to get a general measure of religious power. The mean religious power was 0.308, 0.350, and 0.451 for groups S, K, and R, respectively. A one-way ANOVA resulted in a significant group effect ($F(2,49) = 5.18$, $p < .05$), showing that as the political sophistication increases more political power is attributed to the religious parties in Israel.

Better understanding of the perception of political power may be gained by studying the relationship between the perceived power of different parties over individual subjects. Party size, political ideology, or the history of conflict and cooperation between the parties may result in a positive relationship between the perceived power of certain pairs of parties or a negative relationship for others. To investigate these relationships, all 45 pairwise correlations between perceived power were computed over all 52 subjects. Because the ten weights for each subject sum up to the same constant, the null expectation is for a low negative correlation between parties. Consequently, we chose the significance level of 0.01 for testing the correlations. The results are presented in Table 5.

Table 5 shows several patterns of correlations which are politically interpretable.

1. Regardless of their political ideology, small parties with three or fewer parliament seats (DMC, CRM, MNR, MIT, R) are perceived to resemble one another because of their size. Of the ten correlations in Table 5 between the five "small" parties, eight are positive and the remain-

Table 5. Correlations between Measures of Perceived Power

| | DFP | DMC | CRM | LP | MNR | U | MIT | NRP | AI | R |
|---|---|---|---|---|---|---|---|---|---|---|
| DFP | 1.00 | | | | | | | | | |
| DMC | .38* | 1.00 | | | | | | | | |
| CRM | .60* | .78* | 1.00 | | | | | | | |
| LP | .43* | .14 | .18 | 1.00 | | | | | | |
| MNR | –.03 | .52* | .34 | –.33 | 1.00 | | | | | |
| U | –.26 | –.35 | –.45* | .09 | –.26 | 1.00 | | | | |
| MIT | –.18 | –.01 | –.03 | –.70* | .27 | –.47* | 1.00 | | | |
| NRP | –.29 | –.25 | –.19 | –.72* | .08 | –.43* | .56* | 1.00 | | |
| AI | –.42* | –.22 | –.20 | –.78* | .13 | –.40* | .59* | .73* | 1.00 | |
| R | –.09 | .20 | .24 | –.32 | .31 | –.38* | .41* | .11 | .20 | 1.00 |

$p < 0.01$.

ing two (between DMC and MIT and between CRM and MIT) are practically zero. Of the eight positive correlations, five are significant.

2. Like the five small parties mentioned immediately above, the three religious parties MIT, NRP, and AI are also positively correlated with one another. All three correlations between the religious parties are significant (0.56, 0.59, and 0.73 for MIT and NRP, MIT and AI, and NRP and AI, respectively). Although the three religious parties may compete with one another for the religious vote, they do not compete with each other for power. Rather, they are perceived as constituting one bloc with a relatively high or low power depending on the importance and attention given by the subject to the bargaining tactics used by the religious parties and their ability to satisfy their political demands.

3. The perceived power of each of the religious parties is negatively and significantly correlated with the perceived power of the LP. The correlations between LP and MIT, LP and NRP, and LP and AI are -0.70, -0.72, and -0.78, respectively, each of which accounts for 50% or more of the response variability. Similarly, the correlations between U and each of the three religious parties are also negative and significant (-0.47, -0.43, and -0.40). Finally, the correlation between U and LP (0.09) is practically zero. Politically, this pattern of seven correlations makes much sense because immediately after the 1981 elections a coalition between U and the religious bloc or between LP and the religious bloc was considered much more likely than any other coalition, in particular a national unity coalition including U and LP.

Inspection of Table 5 shows that 14 of the 18 significant correlations are accounted for by patterns 1, 2, and 3 above; the remaining four significant correlations all concern the DFP.

## Spatial Analysis of Proximity Data

In addition to the power ratio judgments, each subject assessed the ideological similarity between any two parties on a 0-100 response scale. The resulting 10 × 10 symmetric proximity matrix of each subject was subjected to a nonmetric multidimensional scaling (Coxon, 1982; Davison, 1983; Kruskal & Wish, 1978; Schiffman, Reynolds, & Young, 1982). A version of ALSCAL (Young & Lewyckyj, 1979) in the SAS computer package was used. For any prespecified value of $m$, the ALSCAL algorithm computes and prints two measures of goodness-of-fit in addition to the $m \times n$ coordinates of the configuration. The RSQ measure is the squared linear correlation between the proximity measures and the corresponding Euclidean distances in the $m$-dimensional configuration. The other measure of goodness-of-fit, denoted by STR, is known as S-STRESS. The present study employed S-STRESS formula one, which is defined in terms of squared distances and squared disparities and is recommended (Davison, 1983) for proximity data. Both the RSQ and STR measures for the two-dimensional spatial solutions are presented in Table 3.

The common criteria of fit, reproducibility, and interpretability were used to determine the appropriate dimensionality of the spatial configurations. For most subjects, a dimension by STR plot began to level off at two dimensions, suggesting that the appropriate solution may be the two-dimensional configuration. Three-dimensional solutions had to be rejected in any case, not because of any belief that political spaces are appropriately interpretable in terms of one or two dimensions only, but because of the recommendation (Young & Lewyckyj, 1979) always to keep the $n/m$ ratio no smaller than 4. With $n = 10$ in the present study, $m \leqslant 2$. The RSQ results also supported the decision that $m = 2$. The mean RSQ measures for groups S, K, and R were 0.857, 0.881, and 0.897, respectively, showing that more than 85% of the variability in the similarity judgments could be explained by the linear relationship between the proximities and the two-dimensional Euclidean distances.

Reproducibility dictates that the spatial solution be composed of dimensions that emerge consistently across subgroups (Davison, 1983). In analyses not reported here the proximity matrices of all the subjects in each group were jointly subjected to an individual differences nonmetric multidimensional scaling, using the ALSCAL program to fit the weighted Euclidean model. The resulting three two-dimensional configurations for groups S, K, and R were practically identical to one another (Golan, 1984, Chap. 6). Moreover, approximately 60% of the individual two-dimensional solutions exhibited more or less the same shape, with most of the parties maintaining their relative positions in the configuration.

Most of the individual two-dimensional configurations as well as the three configurations for groups S, K, and R mentioned above were shaped like a horseshoe, with the ordering of the ten parties on this horseshoe corresponding more or less to their left to right ordering in Table 2. The ordinal positions of adjacent parties in the ordering were often reversed, though, particularly between AI and R and between DMC and CRM. Because no systematic differences among the three group configurations were discovered, all the 52 proximity matrices were jointly subjected to the ALSCAL program. As before, the weighted Euclidean model for individual differences was applied. The resulting two-dimensional solution, also shaped like the same horseshoe, is depicted in Figure 2. Based on several arguments, which are presented elsewhere (Golan, 1984), it was concluded that the solution in Figure 2 (as well as most individual solutions which have a similar shape) is politically interpretable. The horizontal axis may be interpreted in terms of the joint and highly correlated effects of three dimensions, namely, defense policy, economic ideology, and attitude towards religion. The vertical axis is interpreted as a moderation-radicalism dimension, with the more moderate parties placed at the top of the configuration and the more aggressive, traditionally anti-establishment parties placed at the bottom.

### *Model Testing*

In addition to the vector of perceived power $\mathbf{w}$, which was derived from the subject's power ratio judgments, six theoretical vectors were computed. The vectors $\phi$, $\beta$, and $\rho$ are the predictions of the SS, BZ, and DP power indices, respectively. As these indices only consider the seat distribution, they do not allow for individual differences. The other three vectors are $\phi^g$, $\beta^g$, and $\rho^g$. As they also consider the political space, which differs in general from one subject to another, the generalized indices each yield different predictions for different subjects.

Table 6 presents the predicted vectors $\phi$, $\beta$, and $\rho$ for the tenth Knesset. When compared to each other, the two vectors $\phi$ and $\beta$ are seen to be quite similar; the difference $|\phi_i - \beta_i|$ never exceeds 3.5%. The Banzhaf index, though, yields more egalitarian predictions than the Shapley-Shubik index: $\beta_i < \phi_i$ for the two dominant parties LP and U, and $\beta_i > \phi_i$ for the remaining eight, smaller parties. Both these power indices differ radically from the DP index, which predicts more egalitarian results than both. Political considerations would tend to reject the DP offhand, as it prescribes higher voting power to each of the two religious parties NRP and AI and even to the Communist party DFP than to the dominant party U! Clearly the assumptions underlying the DP index do not apply to the tenth Knesset.

Table 7 displays the means and standard deviations of the voting power prescribed by the generalized indices GSS, GBZ, and GDP. Attending first to the GBZ and GDP indices, Table 7 shows relatively small standard deviations of predicted power for each of the ten parties. The ratio of mean to standard deviation for each of these two generalized indices is about 20, indicating very tight frequency distributions of power with small between-subject variability. A comparison of the mean vectors $\beta^g$ and $\rho^g$ in Table 7 with their respective vectors $\beta$ and $\rho$ in Table 6 shows only small discrepancies. The maximal difference between $\beta_i^g$ and $\beta_i$ is 2% (party LP) and the maximal difference between $\rho_i^g$ and $\rho_i$ is 1.7% (party DFP). We conclude from this comparison and the relatively small standard deviations of predicted power that the generalizations of the two power indices BZ and DP had only a marginal effect.

The generalization of the SS index, however, had more pronounced effects. Table 7 shows relatively large standard deviations for the GSS, which exceed the respective means of most of the parties, and a comparison of Tables 7 and 6 also shows marked differences, with the GSS index assigning more power than the SS index to the right-of-the-center parties U, MIT, NRP, and AI and less power to the remaining parties. On the average, the GSS index concentrated most of the voting power (78.7%) in the hands of four parties which, only a short time after the study, actually

**Figure 2. A Two-Dimensional Representation of the Ten Parties Yielded by the INDSCAL Program**

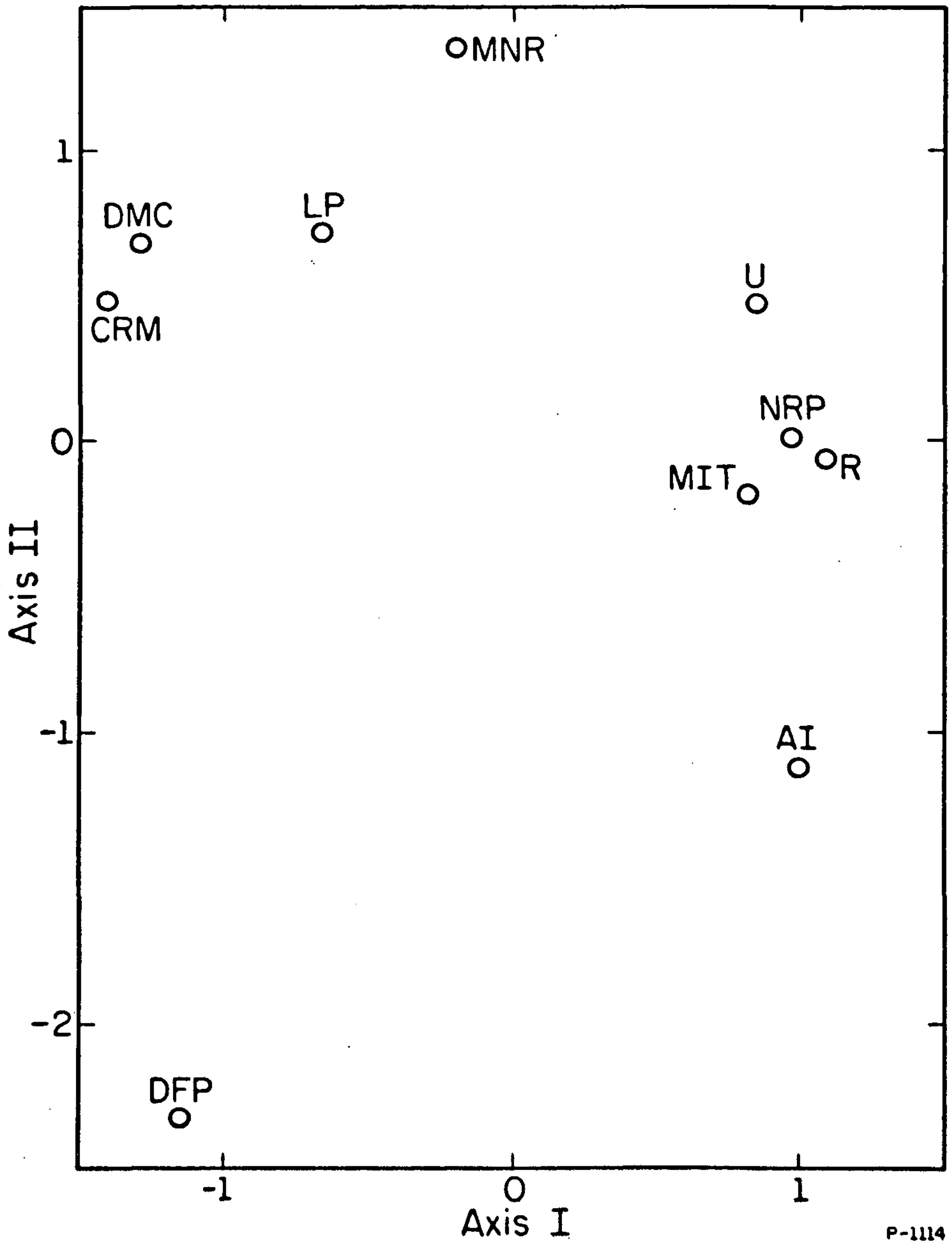


Table 6. Predicted Voting Power by the Shapley-Shubik, Banzhaf, and Deegan-Packel Power Indices

| Party | SS Value $\phi$ | BZ Value $\beta$ | DP Value $\rho$ |
|---|---|---|---|
| DFP | 0.070 | 0.080 | 0.106 |
| DMC | 0.031 | 0.040 | 0.091 |
| CRM | 0.016 | 0.019 | 0.062 |
| LP | 0.264 | 0.229 | 0.105 |
| MNR | 0.031 | 0.037 | 0.091 |
| U | 0.300 | 0.268 | 0.104 |
| MIT | 0.050 | 0.058 | 0.103 |
| NRP | 0.118 | 0.134 | 0.129 |
| AI | 0.070 | 0.080 | 0.106 |
| R | 0.050 | 0.058 | 0.103 |
| Total | 1.000 | 1.003 | 1.000 |

joined forces to form a minimal winning coalition.

Predicted and observed measures of perceived power were next compared to each other by computing for each subject and each power index the root mean squared deviation

$$\sqrt{\sum_{i=1}^{n} (z_i - w_i)^2} \ ,$$

where z = $z_1, \ldots, z_n$) indicates a theoretical vector. The resulting deviation scores, six for each subject, were first used to determine whether the generalizations of the SS, BZ, and DP indices improved the goodness-of-fit. For 49 of the 52 subjects the SS index yielded smaller deviation scores than GSS. A comparison of BZ and GBZ, however, resulted in smaller deviation scores associated with the GBZ index for 39 of the 52 subjects, and in the case of the DP index, the generalization improved the fit in 51 of 52 cases. We next marked for each subject the power index with the smallest deviation score and compared the best models across all the subjects. The GBZ index provided the best fit for 32 of the 52 subjects (61.5%). Figure 3, which plots the mean perceived power for all subjects in Table 4 against the mean predicted power by the GBZ index in Table 7, provides a general idea of how good the fit is. The results of 10 more subjects (30.8%) were best accounted for by the SS index. The two generalized indices GSS and GDP were the most appropriate models for only one and two subjects, respectively.

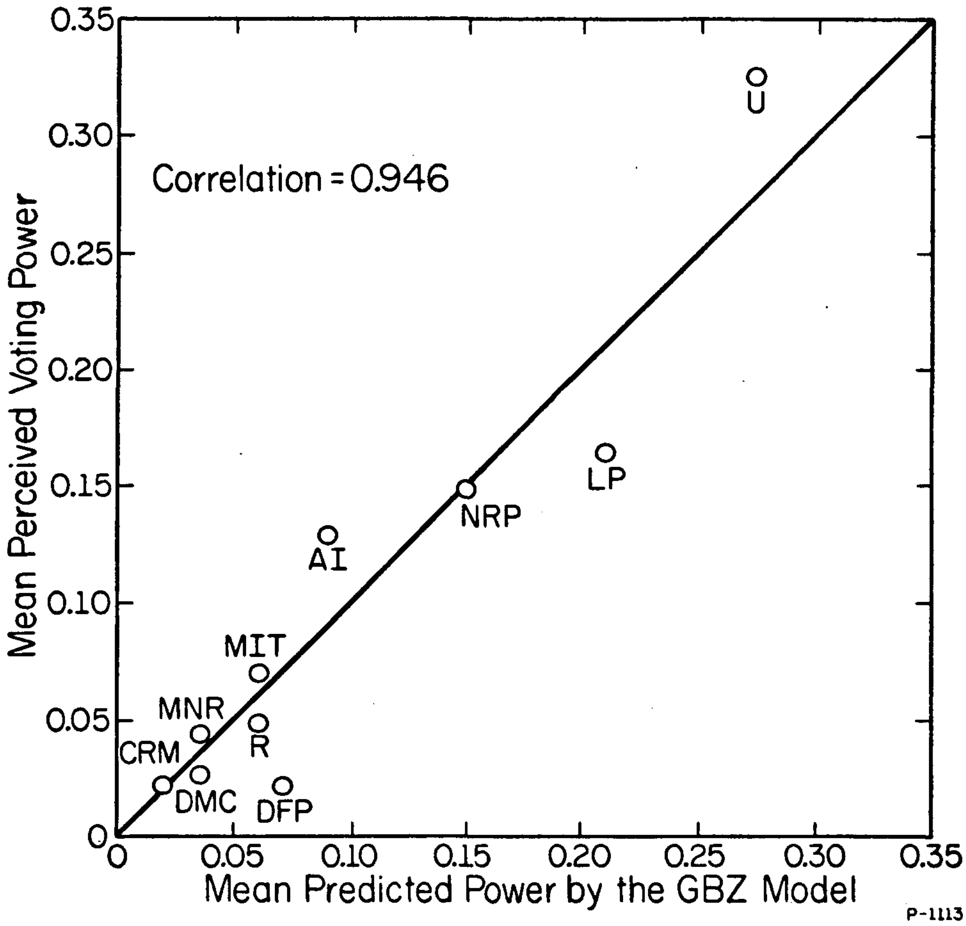
## Discussion

Political power, like beauty, is in the eye of the beholder. It depends on, but is probably not solely determined by, the distribution of parliamentary seats, the ideological space, the personalities involved, party cohesiveness, resources controlled by the party, or the history of conflict and cooperation among the parties involving promises, threats, pressures, horsetrading, betrayal, and

Table 7. Means and Standard Deviations of Predicted Voting Power by the Generalized Power Indices

| Party | GSS Mean | GSS SD | GBZ Mean | GBZ SD | GDP Mean | GDP SD |
|---|---|---|---|---|---|---|
| DFP | 0.008 | 0.019 | 0.071 | 0.003 | 0.089 | 0.007 |
| DMC | 0.010 | 0.018 | 0.036 | 0.002 | 0.085 | 0.005 |
| CRM | 0.007 | 0.014 | 0.019 | 0.001 | 0.056 | 0.003 |
| LP | 0.123 | 0.087 | 0.209 | 0.011 | 0.102 | 0.006 |
| MNR | 0.015 | 0.033 | 0.036 | 0.002 | 0.090 | 0.004 |
| U | 0.421 | 0.246 | 0.272 | 0.011 | 0.111 | 0.006 |
| MIT | 0.054 | 0.099 | 0.060 | 0.003 | 0.109 | 0.004 |
| NRP | 0.165 | 0.201 | 0.148 | 0.007 | 0.139 | 0.005 |
| AI | 0.147 | 0.104 | 0.088 | 0.004 | 0.111 | 0.006 |
| R | 0.049 | 0.080 | 0.060 | 0.003 | 0.108 | 0.004 |
| Total | 0.999 | | 0.999 | | 1.000 | |

**Figure 3. Mean Perceived Power Plotted against the Mean Predicted Power by the GBZ Power Index**



loyalty. Although power is inherently a multidimensional attribute, an implicit assumption of the present study is that individuals can and do integrate some or all of these facets, albeit in different ways, to form a unidimensional scale of political power. Tests of scale reliability, which have not been undertaken in the present study, are required to determine the stability of the integrating function for a given individual over time. The six power indices examined in the present study might be considered, then, as preliminary attempts to model this integrating process. There may be two major reasons for the failure of any of these models to account for perceived political power. The person may not use exactly the same factor or factors that the model postulates, or he or she may use the same factors as postulated but combine them in a different way.

The results support the underlying assumption of the six models that perceived political power is measurable on a ratio scale. Depending on the criterion used to determine deviations from consistency, most of our subjects exhibited an acceptable level of imprecision in their power ratio judgments, which has only a small effect on the derived weights. The analytic hierarchy process was developed by Saaty for the derivation of priorities or weights which reflect the relative importance of the options in a multi-attribute judgment task. There are several advantages to this technique: it is based on pairwise comparisons, which subjects generally find easy to make; it shares with Thurstonian psychophysical scaling technique and signal detectability theory the indirect approach to the analysis of sensory experience and other subjective phenomena; and it yields a numerical measure of response consistency. But the technique also has its shortcomings. The null hypothesis which it rejects in measuring consistency is too weak; there is a need for statistically defined or justified measures of response uncertainty based on stronger assumptions about the null hypothesis than sheer randomness. It also appears (Belton & Gear, 1983) that under certain circumstances the technique may yield anomalous results arising from a misunderstanding of what is required in specifying the outputs. Because of these shortcomings, additional studies are required, which apply concurrently several scaling techniques for the measurement of voting power.

Another finding was that as the political sophistication of the subjects grew, less power was jointly attributed to the two largest parties U and LP, and more power was assigned to the three religious parties MIT, NRP, and AI. The loss of power inflicted on the two dominant parties was approximately equal to the joint gain of power by the religious parties. The mean combined power of U and LP decreased significantly from 0.525 for group S to 0.401 for group R, a drop of 12.4%, compared to an increase of 14.3% in the mean combined power of the three religious parties from 0.308 for group S to 0.451 for group R. We attribute these two compensatory effects to the political experience of the subjects, their historical orientation, and their understanding of parliamentary politics. More research is required to find out whether the effects of political sophistication or experience are restricted to the three populations of subjects examined in the present study or are more general and apply to other segments of the Israeli population.

It was pointed out above that the power indices SS, BZ, and DP share a simple, experimentally testable property, namely, parties with equal numbers of seats are prescribed equal power. The observed results are inconsistent with this prediction. Table 2 shows three pairs of parties with the same number of seats: DFP and AI, MIT and R, and DMC and MNR. But Table 4 shows that for each group separately and over all groups, the AI was attributed between four to six times more power than DFP. Similarly, the mean perceived power of MIT exceeded the mean perceived power of R by about 50%, and the mean perceived power of MNR was almost twice as large as that of DMC. Also noted is the difference in mean perceived power between U and LP, which far exceeds the one seat difference between these two parties. All of these comparisons show that within each pair more power was attributed to the party believed to be the more likely candidate to enter a coalition. They strongly suggest that seat distribution alone cannot account for perceived power and that additional factors, which determine the likelihood of entering a winning coalition, must be considered.

It was, therefore, natural to test the three generalized power indices GSS, GBZ, and GDP, which take into consideration both the seat distribution and the ideological similarities between the parties but combine them in different ways. When compared to the indices SS, BZ, and DP, the generalized indices behaved as expected, prescribing more power to the right-of-center parties and less power to the remaining parties. The DP index and its generalized version GDP are clearly rejected as descriptive models, probably because the assumption of equal division of spoils is untenable. The GSS and GBZ indices "move" in the same direction as the observed results. But although the direction of movement is right, its magnitude is wrong, with the GSS attributing too much power to each of the five right-of-center parties (except MNR), compared with the mean perceived power, and the GBZ index assigning each of them too little power.

Of the different approaches that may be considered in revising the GSS and GBZ indices, one warrants special attention. It may be recalled that both of these indices incorporate the assumption that all issue directions in the ideological space are equally likely. Despite its obvious advantages of simplicity and mathematical tractability, this assumption makes very little political sense. Inspection of voting records in the Knesset shows that the political winds do not blow across the ideological space in a perfectly random way. Rather, the directional distribution of political issues introduced to the Knesset is largely, although not exclusively, determined by the coalition and tends to concentrate around issues which are of major interest to the government. An empirical classification of issues actually voted on by the parliament may help to estimate the distribution of issues in $R^m$, which would then replace the uniform distribution in the computation of the two generalized power indices.

## Appendix:
## Saaty's Ratio Scaling Procedure

Because most social scientists are unlikely to be familiar with Saaty's Analytic Hierarchy Process, a brief discussion is in order. Suppose we wish to compare a set of $n$ objects or activities in pairs according to their relative weights (assumed to belong to a ratio scale). The weights may designate levels of different attributes or dimensions such as importance, desirability, brightness, pleasantness, or power. Denote the objects (political parties in our study) by $C_1, C_2, \ldots, C_n$, and their relative weights (political power) by $w_1, w_2, \ldots, w_n$. The $n \times n$ pairwise comparisons between the $n$ objects may be represented by a matrix **A** of order $n$, known as a reciprocal matrix, in which $a_{ij} = w_i/w_j$ denotes the ratio of the weights of objects $C_i$ and $C_j$, $a_{ji} = w_j/w_i$ is enforced, and 1's are placed along the main diagonal. Thus, only the $n(n-1)/2$ pairwise comparisons $a_{ij}$ are required to construct **A**.

Suppose **A** is known but $\mathbf{w} = (w_1, \ldots, w_n)$ is not, and we wish to recover **w**. To do so, we have to solve the system of equations (in matrix form) $(\mathbf{A} - n\mathbf{I})\mathbf{w} = O$ in the $n$ unknowns $w_1, w_2, \ldots, w_n$, where **I** is the identity matrix and $O$ is a (column) vector of $O$'s. This system has a nonzero solution if and only if $n$ is an eigenvalue (characteristic root) of **A**. Moreover, all the $n$ eigenvalues $\lambda_i$, $i = 1, \ldots, n$, of **A** are zero, except one, called $\lambda_{\max}$, which is equal to $n$. The solution **w** is any column of **A**. The various solutions differ from one another by a multiplicative constant. However, it is desirable to have the solution normalized so that $w_1 + w_2 + \ldots + w_n = 1$.

When the matrix **A** is generated from the known weights (without error), it satisfies the property

$$a_{ik} = a_{ij} \times a_{jk}, \text{ for all } i, j, k = 1, \ldots, n.$$

A matrix **A** that satisfies this property is called *consistent.*

In realistic situations the ratio judgments $a_{ij}$ will be inconsistent to some degree. One subject may judge $a_{ij} = 2$, $a_{jk} = 3$, and $a_{ik} = 5$, whereas another may respond with $a_{ij} = 2$, $a_{jk} = 3$, and $a_{ik} = 4$. We say that the second subject is less consistent than the first. We wish to account for the inconsistency in ratio judgments by deriving from $A$ a single numerical index of the degree of inconsistency in the subject's ratio judgments. Mathematically, this is achieved through the solution of the system of $n$ equations

$$\mathbf{A}\,\mathbf{w} = \lambda_{\max}\,\mathbf{w}, \tag{A1}$$

where both the scalar $\lambda_{\max}$—the maximal eigenvalue of **A**—and the corresponding eigenvector **w** are unknown. It can be shown that a reciprocal matrix with positive entries is consistent if and only if $\lambda_{\max} = n$. With inconsistency, $\lambda_{\max} > n$ always. Saaty has observed that in any square matrix, small perturbations in its coefficients imply small perturbations in the eigenvalues. Hence, the higher the degree of inconsistency in the pairwise comparisons, the higher the difference $\lambda_{\max} - n$.

This observation leads to the next and final step of the scaling procedure, which determines whether the degree of inconsistency observed in the $n(n-1)/2$ pairwise comparisons of the subject is sufficiently small to justify the derivation of a ratio scale. No analytical solution to this ill-defined problem is known, but two different answers have been proposed by Saaty. The first is based on equating inconsistency with randomness. Reverting to Monte Carlo techniques, Saaty (1980, p. 61) computed the sampling distribution of $\lambda_{\max}$ from 500 randomly generated matrices. As in the present study, each matrix was of order 10 × 10 with 1s in the main diagonal entries and with reciprocity enforced. The entries were filled in at random from the scale 1 to 9 (integers only), which is very close to the response scale actually adopted by the subjects of the present study (see below). Saaty reported a truncated normal frequency distribution for $\lambda_{\max}$. For a 1 to 9 response scale and significance level of 0.01, the critical level for rejecting the null hypothesis (by a one-tailed test) is 19.66. The decision rule of this approach is to reject the matrix **A** of pairwise ratio judgments as inconsistent if $\lambda_{\max} > 19.66$ and accept it otherwise. If $\lambda_{\max} < 19.66$, the associated eigenvector—the vector of weights **w**—is computed from Eq. (A1).

Saaty's second answer is to compute the consistency index

$$\mu = (\lambda_{\max} - n)/(n-1), \tag{A2}$$

and to reject **A** as inconsistent if $\mu > \mu_{\text{crit}}$ and accept it otherwise. There is presently no statistical theory that underlies the measure $\mu$. Based on his rich and versatile experience with the procedure, Saaty proposed to fix $\mu_{\text{crit}}$ at 0.1 (1977, 1980) or 0.2 (1983).

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# Multiparty Equilibria under Proportional Representation

JOSEPH GREENBERG
SHLOMO WEBER
*University of Haifa*

*The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation, as well as partially to characterize it. Specifically, let* m *be the quota that represents the minimal number of voters necessary for a candidate to be elected. We show that there is a set of elected candidates, each choosing an alternative and each receiving at least* m *votes, such that no other potential candidate, by offering an additional alternative, can secure at least* m *votes for himself. We then investigate the structure, at equilibrium, of the set of individuals who support a given candidate, as well as study stability properties of the equilibrium. We also provide necessary and sufficient conditions for the equilibrium to consist of a single candidate, thus generalizing Black's median-voter result.*

The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation, as well as partially to characterize it. More specifically, we show that whenever the alternative set is finite and the preference relations of the voters are complete, transitive and single peaked, for any society and any minimum number, $m$, of votes required for a seat, there exists an equilibrium; that is, there is a set of candidates each choosing an alternative and each receiving at least $m$ votes, and moreover, no other potential candidate can secure $m$ votes for himself. In the special case when $m$ is chosen to be greater than half the size of the population, our result yields the existence of a simple majority equilibrium.

In addition, we provide necessary and sufficient conditions for the equilibrium to consist of a single candidate. This result generalizes Black's median voter. For the more general case, when the equilibrium consists of at least two candidates, we prove that there always exists an equilibrium in which there are two candidates whose chosen positions coincide with the peaks of two individuals. (We fully characterize these two individuals.)

We also investigate the structure, at equilibrium, of the set of individuals who support a given candidate. First, by means of an example we show that this set need not be consecutive, that is, it is possible that individuals $i$ and $k$ will support candidate $a$, whereas individual $j$, whose peak lies between the peaks of $i$ and $k$, will vote for candidate $b$. This example also illustrates Black's point that indeed, in contrast to the majority rule where only the peaks of the individuals' preferences matter (the well-known median-voter result), under proportional representation the equilibrium depends on the entire preference relations. We then consider some special cases in which, at equilibrium, the support of each candidate is consecutive. Such is the situation when, for example, each individual's preference relation can be represented by the distance from his peak.

The possibility that, at equilibrium, the support of candidates need not be consecutive might seem, at first sight, somewhat surprising. This phenomenon is, however, quite commonly observed. Indeed, among the individuals who support a conservative party, there might well be voters who are more liberal than some of the supporters of the liberal party. The intuitive reason for this lack of consecutiveness is as follows: All that the phrase "individual $i$ is more conservative than individual $j$" means is that if the individual could dictate his *most preferred* position (ideology), then the one that would be chosen by $i$ is more conservative than the one chosen by $j$. However, when the choice is between two other positions (the parties' ideologies), there is no reason for the above-mentioned phenomenon not to exist. To use an

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analogy, consider two individuals who have a preference ordering over the amount of sugar they wish to have in their tea. Assume that individual *i* likes his tea *best* with one spoon of sugar, while individual *j* likes his best with two spoons. It is, nevertheless, conceivable that when faced with the choice between having tea with no sugar at all or with five spoons of sugar, individual *i* prefers the latter alternative, while individual *j* prefers the former. We illustrate below that there might even be situations in which in *all* equilibria, *all* candidates have a *nonconsecutive* support. It is mainly due to this possibility that the proofs of our results are mathematically rather involved.

Because candidates might well have motives and ambitions to increase the set of their voters, it is interesting to investigate the stability properties of our proposed notion of equilibrium under this alternative assumption, which differs from the one originally imposed (that candidates are equally content with any number of votes above *m*). We therefore define an equilibrium to be stable if there does not exist an elected candidate (i.e., one who currently receives at least *m* votes), who, by deviating from it, is certain to increase the number of his supporters in any new equilibrium that might result. We prove that although not all equilibria are stable, there do exist stable equilibria.

The article is organized as follows. We discuss first the background of the problem, then we present the model and state the main results. The stability analysis and the discussion of "true" or "sincere" versus "manipulative" or "policy oriented" voting follows. The example in the sixth section demonstrates that there are situations in which, at each equilibrium, the support of no candidate is consecutive. Some basic observations on the nature of the equilibrium are given in the seventh section and are then used in the proofs of the results in the concluding sections. Although mathematically quite elementary, these proofs are nevertheless somewhat involved. This is particularly true for the proof of the main result of the study, namely, the existence of an equilibrium under proportional representation.

## Background

Proportional representation in some form is now used in most democracies as the only method of allocating seats for the legislative bodies, municipalities, unions, and other public organizations. Inquiry in regard to scientific methods of electing representative bodies began as early as the latter part of the eighteenth century. (The earliest statement of the proportional ideal apparently was made by Mirabeau in a speech before the Assembly of Provence on January 30, 1789. The earliest published proposal of a proportional system was made by Gergonne in 1820.) Many political philosophers and statesmen advocated its use, believing that

> The only civilized method of democracy is proportional representation in large constituencies returning many members; there is no other method which gives the individual voter a reasonable opportunity of expressing his real preference. It is the right way, and all other ways are wrong and bad. (Wells, 1923)

Multiparty systems—those with three or more major parties—are likely to occur under proportional representation. It is therefore somewhat surprising that in contrast to the vast quantity of literature on the existence and characterization of a (simple) majority equilibrium, where a single party forms the government, very little work has been done on stable multiparty configurations. One possible explanation for this lacuna in the literature might be the difficulties involved in the formal analysis of the proportional representation model, where the solution concept is a *set* of (elected) candidates, rather than a single one, as in the case of majority rule. As noted by Black (1958, p. 76):

> When we were concerned with the election of a single member to represent a constituency, the problem was to find a candidate who stood highest on the average on the preference scales of the voters—and this permitted of several answers being given. In dealing with the statics of P.R. we have the same sort of difficulty in a more acute form; and this is enough to show that no answer that is given could be completely satisfactory.

The proportional representation scheme is also addressed in the classical work of Downs (1957, p. 124), who writes:

> Where proportional representation exists, a party which wins only a small percentage of the total vote may place some of its members in the government, since coalition governments often rule. Thus the minimum amount of support necessary to keep a party going is much smaller than in a plurality system; so a mutiparty system is encouraged. Nevertheless, each party must still obtain a certain minimum number of votes in order to elect members of the legislature who might possibly enter a coalition. For this reason a given distribution of voters can support only a limited number of parties even under proportional representation. Therefore the conditions for equilibrium exist in both two and multiparty systems.

Downs does not, however, provide a formal proof of the existence of such an equilibrium, but argues intuitively:

> There is a limit to the number of parties which can be supported by any one distribution. When that limit is reached, no more new parties can be successfully introduced. The parties extant at that point arrange themselves through competition so that no party can gain more votes by moving to the right than it loses on the left by doing so, and vice versa. The political system thus reaches a state of long-run equilibrium in so far as the number and positions of its parties are concerned, assuming no change in the distribution of voters along the scale." (p. 123)

Thus, all that Downs proves is that *if* there exists an equilibrium, it can support at most an a priori given number of parties, that is, that "parties cannot form ad infinitum" (p. 124, footnote 8). But Downs does not settle the existence issue which, as Black notes, is by no means trivial. As Black (1958, p. 8) states:

> The scheme of P.R. which we have outlined is too primitive mathematically to be satisfactory—it depends only on the position of the peaks of the curves concerned and does not take into account the entire constellation of the curves. In this respect it contravenes one of the requirements which we have considered necessary in a solution. Our purpose in considering it, indeed, has been to illustrate the difficulties of getting any satisfactory scheme of P.R. of a mathematical nature, even in the simplest of cases.

The purpose of this work is to define formally and to analyze the notion of an equilibrium under proportional representation. (To the best of our knowledge, no rigorous treatment of multiparty equilibria has been offered before.) There are, however, many methods of securing proportionality that can be and have been used. (For example, see Hoag & Hallett, 1926, pp. 412-456.) The main difference among the various systems of proportional representation is in the way the remainder of the vote is allocated. For this work we chose to analyze the Uniform Quota system, which was used in Germany during the Weimar Republic, and is similar in principle to d'Hondt rule, which is currently used for parliamentary elections in several European countries and in Israel.

## The Uniform Quota

Hoag and Hallett (1926, pp. 412-414) describe and discuss the uniform quota method as follows:

> The simplest method of apportioning the seats to the lists is that used for the election of the Reichstag under the new republican constitution of Germany. An arbitrary quota is fixed for all districts. In German Reichstag elections it is fixed permanently at 60,000. In each district each party is assigned as many seats as there are quotas in its vote.
>
> The "uniform quota" method has some obvious advantages. It tends to interest all elements in securing full participation in elections, since the representation of each district as well as of each point of view within each district depends entirely on the number of quotas of votes it polls. It puts elected members from all parties and all parts of the country on an equal footing, since the election quota is everywhere the same. And it gives every voter the unique privilege of not helping to secure additional representation for his district if he cannot secure the kind of representation he wants. If applied to congressional elections in our southern states, where only Democrats are now elected, for example, it would relieve the Republicans who are not allowed to vote of the necessity of being counted to give additional seats to their states in the apportionment and thus really of providing more seats for the Democrats. In states like North Carolina, where considerable numbers of Republicans vote, it would of course—like any plan of proportional representation—enable them to elect members of their own.
>
> The main objection to the uniform quota principle is that it causes fluctuations not only in the size of the district delegation but in the size of the whole body elected. And these fluctuations may depend not only on fluctuations in interest but on such fortuitous circumstances as whether it rains on election day. Furthermore, if it rains in some parts of the country but not in others, the former may be relatively underrepresented. Such considerations may be of minor importance, but nearly all proportional representation countries have adhered to the long-established practice of assigning a definite number of members to each district in advance.

Another example in which the quota is fixed in advance instead of the number of members to be elected is the Israeli law for candidates running for the Knesset (the Israeli parliament). According to that law, any person who wishes to run for the Knesset has to produce a given number (quota) of signatures of citizens who are eligible to vote and who support his candidacy. In the recent 1984 elections, this quota was 5,000 signatures.

Although we know of no other examples in which the uniform quota is, or was, used, we wish to point out that the appointing method devised by the Belgian mathematician d'Hondt is very closely related to the uniform quota. As Hoag and Hallett note (1926, pp. 421-422):

> In any of its forms the d'Hondt method is similar in principle to the uniform quota method. It gives a uniform quota assignment for each dis-

trict to which it is separately applied, the only difference being that the quota is determined so as to give a desired total membership instead of being fixed arbitrarily.

> In Europe it appears to be generally considered the fairest of the methods of apportionment used for list system elections for a definite number of members, as is evidenced by its widespread use. It is the most strictly proportional method possible for such elections in the somewhat restricted sense that if any list wins a seat for a given number of votes, every other list is sure to win at least as many seats as the number of times its vote contains that number. No other method used or proposed for such elections satisfies that criterion.
>
> (The one objection frequently urged against the d'Hondt principle is that it is likely to give a large party a fraction of a seat more than its exact proportion and a small party a fraction of a seat less, thus tending to discourage independent groups from nominating separate lists.)

Finally, it is interesting to note that the first published proposal of proportional representation apparently advocated the uniform quota system. In 1820 the French mathematician Gergonne published in the *Annales de Mathematiques* an article entitled "Arithmetique politique. Sur les elections et le systeme representatif," in which he said: "At the elections the voters would group themselves freely according to their opinions, their interests, or their desires, and any citizen would become a Deputy from the department in the elective chamber who bore a mandate from two hundred voters.

## The Model

Society is represented by the finite set $N = \{1,2, \ldots, n\}$ of $n$ voters. Voter $i \in N$ has a preference relation, denoted by $\geqslant_i$, over a finite set of alternatives, $\Omega$, where for every $a,b, \in \Omega$, $a \geqslant_i b$ is interpreted to mean that voter $i$ (weakly) prefers $a$ to $b$, and $a >_i b$ means that voter $i$ strictly prefers $a$ to $b$. Since $\Omega$ is finite, the alternatives in $\Omega$ can be ordered (assigned numbers) so that for every two alternatives $a,b \in \Omega$ either $a > b$ or $b > a$. Given this ordering of the alternatives in $\Omega$, we assume that the preference relation of each individual $i \in N$ is:

ASSUMPTION 1. Complete. That is, for every two alternatives $a,b \in \Omega$, either $a >_i b$ or $b >_i a$.

ASSUMPTION 2. Transitive. That is, for every three alternatives $a,b,c, \in \Omega$: If $a >_i b$, and $b >_i c$, then $a >_i c$.

ASSUMPTION 3. Single peaked. That is, for each $i \in N$, there exists a unique alternative $q(i) \in \Omega$, called $i$'s peak, such that for every two alternatives $a,b \in \Omega$, if either $q(i) \geqslant a > b$ or $q(i) \leqslant a < b$, then $a >_i b$.

*Without loss of generality, let the voters be numbered (named), so that* $q(1) \leqslant q(2) \leqslant \ldots \leqslant q(n)$. That is, individual 1's most preferred alternative is to the left of that of Mr. 2's which is, in turn, to the left of Mr. 3's, and so on. The rightmost peak is that of Mr. $n$.

The exogenously given positive integer $m$, $m \leqslant n$, represents the least amount of votes required for one seat in the parliament. Given society $N$, the set $\Omega$ and the number $m$, we define an *m-equilibrium* as a set of alternatives such that when the voters are faced with the choice among these alternatives, and when each voter votes for the alternative he prefers best in this set, each alternative receives at least $m$ votes and, moreover, no new (potential) candidate can attract $m$ voters by offering another alternative, in addition to those offered in the $m$-equilibrium.

In order to define formally an $m$-equilibrium we introduce the following notation and definitions which will be used throughout the article:

$N = \{1,2, \ldots, n\}$,
$A = \{a_1 \ldots, a_T\}$ where $a_t \in \Omega$
for all $t = 1, \ldots, T$, $a_1 < a_2 \ldots < a_T$
$m$ – a positive integer not exceeding $n$.

DEFINITION 1. The *support* of alternative $a \in \Omega$ given $N$ and the subset of alternatives $A$, denoted $S(a; N,A)$, is the set of all individuals who strictly prefer alternative $a$ over any other alternative in $A$. That is, $S(a; N,A) = \{i \in N | a >_i a_t \text{ for all } a_t \in A, a_t \neq a\}$. The cardinality of $S(a; N,A)$ is denoted $s(a; N,A)$.

DEFINITION 2. The support $S(a; N,A)$ is called *consecutive* if for any three individuals $i,j,k$ with $i < j < k$, $i,k \in S(a; N,A)$ implies that $j \in S(a; N,A)$.

That is, $S(a; N,A)$ is consecutive if whenever Mr. $i$ and Mr. $k$ prefer alternative $a$ to any other alternative in $A$, so will all other individuals whose peaks lie between those of individuals $i$ and $k$. (As we shall see below, perhaps somewhat unexpectedly, the support need not be consecutive.)

Using the above terminology we have that the collection $A$ is an $m$-equilibrium for $N$ if the support of each alternative in $A$ consists of at least $m$ individuals (thus, each elected candidate receives at least $m$ votes), whereas no other alternative is supported (given $A$) by $m$ or more voters; thus, no potential candidate can guarantee himself a seat. Formally, DEFINITION 3. A set of alternatives $A$ is called an *m-equilibrium for N* (or, simply, *an m-equilibrium*) if: for all $a \in \Omega$, $s(a; N,A) \geqslant m$ if and only if $a \in A$.

*The set of all m*-equilibria for $N$ is denoted by $E(N,m)$.

DEFINITION 4. A set $A$ is called a *consecutive m-equilibrium for N* (or, simply *a consecutive m-equilibrium,* if $A \in E(N,m)$ and for every $a \in A$, $S(a; N,A)$ is consecutive.

The set of all consecutive $m$-equilibria for $N$ is denoted by $E_c(N,m)$.

We can now state our main results. Let $N$ be any finite society, which consists of $n$ individuals, let $\Omega$ be any finite set of alternatives, and let $m$ be any positive integer not exceeding $n$. Moreover, we shall assume throughout that Assumptions 1-3 hold.

The first result states that there are cases in which an $m$-equilibrium exists but is not consecutive. It is primarily this possibility that makes the proofs of our results rather involved. Formally,

*Result 1.* It is possible that there exists no consecutive $m$-equilibrium, that is, $E_c(N,m)$ might be empty, and moreover, that for any $A \in E(N,m)$ and any $a \in A$ the support $S(a; N,A)$ is not consecutive.

The second result is the main conclusion of the article, namely, that an $m$-equilibrium always exists.

*Result 2.* For any $m$, $0 < m \leqslant n$, there exists an $m$-equilibrium.

The next three results serve as auxiliary lemmata for the proof of Result 2, but they are of independent interest as they provide a partial characterization of the $m$-equilibria. Result 3 describes fully the set of $m$-equilibria in the special case in which it consists of a single element (i.e., one candidate wins the election). In order that one alternative constitute an $m$-equilibrium the peak of individual $m$ must lie to the right of that of individual $n - m + 1$. Moreover, any alternative between (and including) these two peaks constitutes an $m$-equilibrium. Formally,

*Result 3.* The following three statements are equivalent:

(i) There exists an $m$-equilibrium which consists of a single alternative;

(ii) $q(m) \geqslant q(n - m + 1)$;

(iii) For every $\alpha$, $q(m) \geqslant \alpha \geqslant q(n - m + 1)$, $\{\alpha\}$ is an $m$-equilibrium.

Note that Result 3 generalizes Black's median voter result for the (strict) majority rule. Indeed, if the quota for a seat exceeds half the population, then $m > n/2$, implying that $m \geqslant n - m + 1$ and hence $q(m) \geqslant q(n - m + 1)$. By (iii), the median voter's peak is a majority equilibrium. Observe also that (i) does *not* exclude the possibility that there are $m$-equilibria that consist of two alternatives, even when $q(m) \geqslant q(n - m + 1)$.

Consider an individual that joins a society $N$ which has an $m$-equilibrium that consists of a single alternative. Suppose that the peak of that individual is (weakly) to the right of all the peaks of the individuals in $N$. Then, Result 4 asserts that either $q(m)$ or $\{q(m), q(n - m + 2)\}$ constitutes an $m$-equilibrium for the enlarged society. If it is the latter, by (i) of Result 3 there is no peak that lies between $q(m)$ and $q(n - m + 2)$. Hence, in either case the $m$-equilibrium is consecutive. Formally,

*Result 4.* Let $\tilde{N} = \{1,2, \ldots, n+1\}$. If $q(m) \geqslant q(n - m + 1)$, then either there exists an $m$-equilibrium for $\tilde{N}$ which consists of a single alternative or else $\{q(m), q(n - m + 2)\}$ is an $m$-equilibrium for $\tilde{N}$. Hence, the set of consecutive $m$-equilibria for $\tilde{N}$ is nonempty.

Note that Result 4 does *not* rule out the existence of a nonconsecutive $m$-equilibrium for $\tilde{N}$.

Result 5 gives a partial characterization of the set of $m$-equilibria that consist of more than a single alternative. Specifically, in that case there always exists at least one $m$-equilibrium that contains the following two peaks: that of individual $m$ and that of individual $n - m + 1$. Moreover, these alternatives are, respectively, the first and last elements of that $m$-equilibrium.

*Result 5.* If $q(m) < q(n - m + 1)$ then there exists an $m$-equilibrium $A = \{\alpha_1, \ldots, \alpha_T\}$ with $\alpha_1 = q(m)$ and $\alpha_T = q(n - m + 1)$.

In the special case that disutility is determined by distance, we have:

*Result 6.* If the preference ordering of every individual can be represented by the distance of his peak to the alternative, i.e., for each $i \in N$ and every $a, b \in \Omega$, $a \succ_i b$ if and only if $|a - q(i)| < |b - q(i)|$, then there exists a consecutive $m$-equilibrium. Moreover, in this case every $m$-equilibrium is a consecutive $m$-equilibrium.

## Discussion and Stability

The purpose of this article is to define and to prove formally the existence of an equilibrium under proportional representation. In view of "the difficulties of getting any satisfactory scheme of proportional representation of a mathematical nature, even in the simplest of cases" (Black, 1958, p. 80), as is indeed evidenced by the complexity of the proofs of our results, it seems that Definition 3 and Results 1-6, in particular Result 2, have accomplished that goal (at least as a first step).

It might, therefore, be useful to pause and to examine the proposed equilibrium concept more closely, before proceeding with the proofs of the results. Recall that an $m$-equilibrium consists of a set of alternatives, each offered by an elected candidate, so that each receives at least $m$ votes and there is no other candidate that can attract at least $m$ voters by offering an additional alternative. There are two points that deserve, perhaps, an explanation or a defense.

The first point is that it is assumed that individuals vote sincerely and not manipulatively in the sense of policy-oriented voting; that is, each individual votes for that candidate who best represents his preferences, and voters do not ask about the effect of their votes on the ultimate policy choice of the government. As is well known, this criticism is justified provided that only some of the elected candidates form a coalitional government, and when the number of voters that support an elected candidate influences his probability to belong to the government. Even in that case our proposed notion of an equilibrium serves as a reference point from which voters might deviate. However, in order to circumvent this difficulty completely, assume that all of the elected candidates form a committee (or the cabinet) with each having one vote and decisions are made according to some majority rule. In that case, faced with a set of candidates that forms an $m$-equilibrium, each voter will indeed find *sincere* voting to be his optimal strategy. Moreover, as we shall see below, even when the elected candidates "are weighted" by the number of their constituents, it is still best for each voter, when faced with a set of alternatives that is a (stable) $m$-equilibrium, to vote candidly, namely, for the alternative that ranks highest, within this set, according to his true preference relation.

The second point is that candidates are assumed to be satisficers rather than maximizers; that is, all that a candidate wishes is to receive the quota of $m$ votes, but he is not trying to maximize the number of his supporters. Indeed, even at an $m$-equilibrium, an elected candidate might, by offering an alternative that differs from the one he offers at the equilibrium, increase the number of individuals who vote for him. In that sense, therefore, the equilibrium notion is not stable since elected candidates might wish to deviate from the positions they currently hold.

As candidates do indeed have motives and ambitions to increase the set of individuals that support them, we find this point worth pursuing. Therefore, rather than defend the satisficing criterion, we will define and establish the existence of a stable equilibrium, where no candidate can benefit by deviating from it.

Since at an $m$-equilibrium no potential candidate can receive $m$ votes, nonelected candidates cannot upset it. It follows that the only possible source for the instability of the equilibrium is that an elected candidate might wish to deviate from the position he currently holds. Clearly, every elected candidate is aware that if he deviates, other candidates (elected as well as potential) can do the same. His deviation might cause a chain reaction which the elected candidate must take into consideration. Hence, an elected candidate will have an incentive to deviate from the equilbrium only if he is certain that no matter which new equilibrium will result from his deviation, his set of voters will increase. A stable equilibrium is one from which no candidate has an incentive to deviate.

More specifically, let $A = \{a_1, \ldots, a_T\}$ be an $m$-equilibrium and assume that the elected candidate $t$ considers offering an alternative $w \in \Omega$ instead of $a_t$. He realizes that such a move will bring about a new $m$-equilibrium, denoted by $B$, in which he will offer $w$. (The new equilibrium $B$ might, for example, be $B = \{a_1, \ldots, a_{t-1}, w, a_{t+1}, \ldots, a_T\}$. In other words, in the spirit of a Nash equilibrium, an elected candidate who contemplates a deviation assumes that all the elected candidates will not change their required strategies in response to his deviation. But because our result is sufficiently general, no particular assumptions on the form of $B$ need be imposed.) Therefore, offering $w$ rather than $a_t$ will benefit the elected candidate $t$ only if the number of individuals who support him at $B$ is greater than the number of his supporters at $A$. Formally,

DEFINITION 5. Let $A = \{a_1, \ldots, a_T\}$ be an $m$-equilibrium. An elected candidate $t$ *induces* $B$ from $A$ via $w$ if $B$ is the $m$-equilibrium that results from $A$ when the elected candidate $t$ offers alternative $w$ rather than $a_t$, and $s(w; N,B) > s(a_t; N,A)$.

We shall say that $B$ is *induced* from $A$ if there exists an elected candidate $t$ and an alternative $w$ such that $t$ induces $B$ from $A$ via $w$.

But candidate $t$, when inducing $B$ from $A$, must take into account that at $B$ another candidate might contemplate the inducement of yet another new equilibrium, $C$, and so on. This chain reaction of a sequence of induced equilibria is formalized by:

DEFINITION 6. Let $A = \{a_1, \ldots, a_J\}$ be an $m$-equilibrium. An elected candidate $t$ *generates* $B$ from $A$ via $w$ if there exists a finite sequence $A_1, \ldots, A_J$ with the following properties:

(i) $t$ induces $A_1$ from $A$ via $w$.

(ii) $A_J = B$.

(iii) For all $j = 2, \ldots, J$, $A_j$ is induced from $A_{j-1}$.

Now, given an equilibrium $A$, candidate $t$ will offer $w$ instead of $a_t$ only if in any equilibrium that might result from his deviation, that is, in every $m$-equilibrium that he generates, the number of his supporters is greater than the number of individuals that vote for him at the original equilibrium $A$. Formally,

DEFINITION 7. Let $A = \{a_1, \ldots, a_T\}$ be an $m$-equilibrium. An elected candidate $t$ has an *incentive to deviate* from $A$ via $w$ if in all $B$ that $t$ generates from $A$ via $w$, the number of individuals who vote for him is greater than $s(a_t; N,A)$.

Finally,

DEFINITION 8. An $m$-equilibrium $A$ is *stable* if no elected candidate has an incentive to deviate from it via some $w$ in $\Omega$.

With the above definitions we can now formally state our two results on stable equilibria.

*Result 7.* Not every $m$-equilibrium is stable.

*Result 8.* For any $m \leqslant n$ there exists a stable $m$-equilibrium. Moreover, Results 1-6 are also true when restricted to the set of stable $m$-equilbria.

### Proof of Result 1

It is sufficient to consider the following example:

$N = \{1, 2, \ldots, 8\}$ (i.e., $n = 8$)
$\Omega = \{a,b,c,d\}$ where $a < b < c < d$
$m = 3$. The preferences of the individuals are:

$a \succ_i b \succ_i c \succ_i d$ for $i = 1,2,3$;
$b \succ_i c \succ_i d \succ_i a$ for $i = 4$;
$c \succ_i b \succ_i a \succ_i d$ for $i = 5$;
$d \succ_i c \succ_i b \succ_i a$ for $i = 6,7,8$.

It is easy to check that these preferences satisfy assumptions 1-3 and that individuals are named so that $q(1) = q(2) = q(3) < q(4) < q(5) < q(6) = q(7) = q(8)$. Now every $m$-equilibrium, if it exists, must contain alternatives $a$ and $d$, since each of these two alternatives is always supported by at least three individuals, and $m = 3$. Moreover, because $n = 8$ and $m = 3$, no subset of $\Omega$ which contains more than two alternatives can be an $m$-equilibrium. Thus, the only candidate for an $m$-equilibrium is the set $A = \{a,d\}$. To conclude the proof of Result 1, it is sufficient to show that both $S(a;\ N,A)$ and $S(d;\ N,A)$ contain at least three individuals but neither set is consecutive. Indeed, since $a \succ_5 d$ and $d \succ_4 a$ we have that $S(a;\ N,A) = \{1,2,3,5\}$ and $S(d;\ N,A) = \{4,6,7,8\}$.

Q.E.D

### Basic Observations

The following seven observations are straightforward and will be used in the proofs of the main results. They might be of independent interest and increase the familiarity with the concepts of $m$-equilibrium and the support of an alternative. Recall that $A$ is an ordered subset of $\Omega$ (although not necessarily an $m$-equilibrium); that is, $A = \{a_1, \ldots, a_T\}$ with $a_1 < a_2 < \ldots < a_T$ and $a_t \in \Omega$ for all $t = 1, \ldots, T$.

The first observation states that an individual $i$ whose peak $q(i)$ is to the right of an alternative $a_t$ in $A$ will prefer $a_t$ to any alternative that lies to its left. Thus, such an individual will never support a candidate who proposes an alternative $b < a_t$ if the set $A$ is also offered to him. Formally,

OBSERVATION 1. Let $b \in \Omega$. If for some $a_t \in A, b < a_t$, then $a(i) \geqslant a_t$ implies $i \notin S(b;\ N,A)$. In particular, if $a_1 \leqslant q(m)$, then $s(b;\ N,A) < m$ for all $b < a_1$: Indeed, let $q(i) \geqslant a_t > b$. Then the single peakedness implies that $a_t \succ_i b$, thus $i \notin S(b;\ N,A)$. In particular, therefore, if $a_1 \leqslant q(m)$ then $j \geqslant m$ implies that $j \notin S(b;\ N,A)$ for all $b < a_1$.

Observation 2 is the dual of Observation 1. It asserts that if individual $i$'s peak is to the left of an alternative $a_t \in A$, then he will not support any candidate that proposes an alternative $b > a_t$, whenever it is feasible for $i$ to choose also from the set $A$. Formally,

OBSERVATION 2. Let $b \in \Omega$. If for some $a_t \in A$, $b > a_t$, then $q(i) \leqslant a_t$ implies $i \notin S(b;\ N,A)$. In particular if $a_T \geqslant q(n - m + 1)$, then $s(b;\ N,A) < m$ for all $b > a_T$: Indeed, let $b >$

$a_t \geqslant q(i)$. Then the single peakedness implies that $a_t >_i b$, thus $i \notin S(b; N,A)$. In particular, therefore, if $a_T \geqslant q(n - m + 1)$ then $j \leqslant n - m + 1$ implies that $j \notin S(b; N,A)$ for all $b > a_T$.

An immediate application of Observations 1 and 2 yields:

OBSERVATION 3. Let $b \in \Omega$ and $i \in S(b; N,A)$. Then, $q(i) < a_t$ for all $a_t \in A$, $a_t > b$, and $q(i) > a_t$ for all $a_t \in A$ that satisfies $a_t < b$.

OBSERVATION 4. $S(a_1; N,A) \supset \{i \in N | q(i) \leqslant a_1\}$ and $S(a_T; N,A) \supset \{i \in N | q(i) \geqslant a_T\}$. In particular, if $a_1 = q(m)$, then $s(a; N,A) \geqslant m$, and if $a_T = q(n - m + 1)$, then $s(a_T; N,A) \geqslant m$.

Let $a$ be an alternative in $\Omega$ that differs from individual $i$'s peak, $q(i)$. Then, any alternative that lies between $q(i)$ and $a$ which differs from $a$ will be preferred by individual $i$ over $a$. It follows that if alternative $a$ is individual $i$'s best choice among the elements in $A$, and $b$ is an alternative that lies between $q(i)$ and $a$, $b \neq a$, then individual $i$'s best alternative in the set $A \cup \{b\}$ is alternative $b$. Formally,

OBSERVATION 5. If $i \in S(a; N,A)$ and $q(i) < a$, then $i \in S(b; N,A)$ for all $b$ with $q(i) \leqslant b < a$: Indeed, let $q(i) \leqslant b < a$. Because $i \in S(a; N,A)$, $a >_i a_t$ for each $a_t \in A$, $a_t \neq a$. By the single peakedness, $b >_i a$, thus the transitivity implies that $b >_i a_t$ for each $a_t \in A$, that is, $i \in S(b; N,A)$.

Let $a_t$, $a_{t+1}$, $b_h$, and $b_{h+1}$ be four alternatives in $\Omega$ that satisfy: $b_h \leqslant a_t < a_{t+1} \leqslant b_{h+1}$. Let $c \in \Omega$ be an alternative that lies strictly between $a_t$ and $a_{t+1}$. Observation 6 asserts that if individual $i$ prefers $c$ to both $a_t$ and $a_{t+1}$, then he will also prefer $c$ to both $b_h$ and $b_{h+1}$. Formally,

OBSERVATION 6. Let $B = \{b_1 \ldots, b_H\}$ where $b_h \in \Omega$ for each $h = 1,2, \ldots, H$, and $b_1 < b_2 < \ldots < b_H$. Assume that for some $t$ and $h$, $b_h \leqslant a_t$ and $b_{h+1} \geqslant a_{t+1}$. Then for all $c$ with $a_t < c < a_{t+1}$, $S(c; N,A) \subset S(c; N,B)$. In particular, if $b_h = a_t$ and $b_{h+1} = a_{t+1}$, $S(c; N,A) = S(c; N,B)$ for each $c$ satisfying $a_t < c < a_{t+1}$: Indeed, let $a_t < c < a_{t+1}$ and let $i \in S(c; N,A)$. Then by Observation 3, $a_t < q(i) < a_{t+1}$. There are two possible cases: $a_t < c \leqslant q(i)$ $a_{t+1}$ and $a_t < q(i) \leqslant c < a_{t+1}$. Because $c >_i a_t$ and $c >_i a_{t+1}$ the single peakedness implies that in both cases $c >_i b_h$ and $c >_i b_{h+1}$. Thus, $i \in S(c; N, \{b_h, b_{h+1}\})$ which coincides, by Observation 3, with $S(c; N,B)$, i.e., $i \in S(c; N,B)$.

Observation 7 states that in any $m$-equilibrium the (first) leftmost alternative lies (weakly) to the left of individual $m$'s peak and the (last) rightmost alternative lies (weakly) to the right of $q(n - m + 1)$. That is,

OBSERVATION 7. Let $A \in E(N,m)$. Then, $a_1 \leqslant q(m)$ and $a_T \geqslant q(n - m + 1)$: Indeed, if $a_1 > q(m)$, then by Observation 4 $s(q(m); N,A) \geqslant m$, thus by Definition 3 $A \notin E(N,m)$. If $a_T < q(n - m + 1)$ then by Observation 4 $s(q(n - m + 1); N,A) \geqslant m$, thus by Definition 3, $A \notin E(N,m)$.

## Proofs of Results 3 and 4

Given a society $N = \{1,2, \ldots, n\}$ we shall denote, throughout, $\tilde{N} = \{1,2, \ldots, n + 1\}$, $= N \cup \{n+1\}$ where $q(n + 1) \geqslant q(n)$.

### Proof of Result 3

We shall show that (i) implies (ii), (ii) implies (iii), and (iii) implies (i).

(i) → (ii): Indeed, let $\{\alpha\} \in E(N,m)$. Then, by Observation 7, $\alpha \leqslant q(m)$ and $\alpha \geqslant q(n - m + 1)$, thus $q(m) \geqslant q(n - m + 1)$.

(ii) → (iii): Indeed, assume that $q(m) \geqslant q(n - m + 1)$, and let $q(m) \geqslant \alpha \geqslant q(n - m + 1)$ Then, by Observation 1, $s(b; N,\{\alpha\}) < m$ for all $b < \alpha \leqslant q(m)$, and by Observation 2, $s(b; N,\{\alpha\}) < m$ for all $b > \alpha \geqslant q(n - m + 1)$. Since $s(\alpha; N,\{\alpha\}) = n \geqslant m$, by Definition 3, $\{\alpha\} \in E(N,m)$.

(iii) → (i): Trivial.

Q.E.D.

### Proof of Result 4

Assume that $q(m) \geqslant q(n - m + 1)$.

If, moreover, $q(m) \geqslant q(n - m + 1)$, then by Result 3, $\{b\} \in E(\tilde{N},m)$ for all $b$ that satisfies $q(m) \geqslant b \geqslant q(n - m + 2)$. Since $S(a; N,\{a\}) = N$ for all $a \in \Omega$, $E_c(N,m) \neq \phi$, and the Result holds.

Suppose, therefore, that $q(n - m + 2) > q(m) \geqslant q(n - m + 1)$, and let $D = \{q(m), q(n - m + 2)\}$. Since $q(m) \geqslant q(n - m + 1)$, $\{i \in \tilde{N} | q(m) < q(i) < q(n - m + 2)\} = \{i \in N | q(m) < q(i) \leqslant q(n - m + 1))\} = \phi$. Hence, by Observation 3, $s(c; \tilde{N},D) = 0 < m$ for all $c$ that satisfies $q(m) < c < q(n - m + 2)$. By Observations 1 and 2 we have that $s(c; N,D) < m$ for both $c < q(m)$ and $c > q(n - m + 2)$. Since by Observation 4 $s(q(m); \tilde{N},D) \geqslant m$ and $s(q(n - m + 2); ND) \geqslant m$, Definition 3 implies that $D \in E(N,m)$. Moreover, by Observation 4, $S(q(m); N,D) \supset$

$\{1, \ldots, m\} \supset \{1, \ldots, n - m + 1\}$ and $S(q(n - m + 2); \tilde{N},D) \supset \{n - m + 2, \ldots, n + 1\}$. Thus, $S(q(m); N,D) = \{1, \ldots, n - m + 1\}$ and $S(q(n - m + 2); \tilde{N},D) = \{n - m + 2, \ldots, n + 1\}$, that is, $D$ is a consecutive $m$-equilibrium for $\tilde{N}$.

Q.E.D.

## Proofs of Results 2, 5 and 6

Both Results 2 and 5 are corollaries of the following theorem.

THEOREM. For two positive integers $n,m$ with $n \geqslant m$ either there exists an $m$-equilibrium which consists of a single alternative or else there exists an $m$-equilibrium $\{\gamma_1, \ldots \gamma_T\}$ where $\gamma_1 = q(m)$ and $\gamma_T = q(n - m + 1)$.

We present the proof of the Theorem after establishing the following fact. Let $A$ be an $m$-equilibrium for $N$ that consists of at least two alternatives, the first being $q(m)$ and the last being $q(n - m + 1)$. Then, if by replacing $q(n - m + 1)$ in $A$ by $q(n - m + 2)$ the resulting subset is not an $m$-equilibrium for $N$, then it must be the case that a potential candidate can attract at least $m$ votes by offering an alternative that lies between $a_{T-1}$ and $q(n - m + 1)$ which differs from $a_{T-1}$. Formally,

LEMMA 1. Let $A \in E(N,m)$ where $T \geqslant 2$, $a_1 = q(m)$, $a_T = q(n - m + 1)$ and let $\tilde{a}_T = q(n - m + 2)$. Then $\tilde{A} = \{a_1, \ldots, a_{T-1}\ \tilde{a}_T\} \notin E(\tilde{N},m)$ if and only if there exists $b$, $a_{T-1} < b \leqslant a_T$ such that $s(b; \tilde{N},\tilde{A}) \geqslant m$.

*Proof.* By Observation 6, for all $a < a_{T-1}$, $S(a; N,\tilde{A}) = S(a; N,\tilde{A})$ and $S(a_{T-1}; \tilde{N},\tilde{A}) \supset S(a_{T-1}; \mathrm{N},\tilde{\mathrm{A}}) \supset S(a_{T-1}; N,A)$. Since $A \in E(N,m)$, by Definition 3 for all $a \leqslant a_{T-1}$, $s(a; \tilde{N},\tilde{A}) \geqslant m$ if and only if $a \in \tilde{A}$. Therefore, Observations 2 and 4 imply that $\tilde{A} \in E(\tilde{N},m)$ if and only if for all $a$, $a_{T-1} < a \leqslant \tilde{a}_T$, $s(a; N,A) < \mathrm{m}$. By Observation 2, for all $a$, $a_{T-1} < a \leqslant \tilde{a}_T$, $S(a; N,A) \subset \{1, \ldots, n - m + 1\}$. Thus, by Observation 5, for all $a$, $a_{T-1} < a < \tilde{a}_T$, $S(a; N,A) = S(a_T; \tilde{N},\tilde{A})$. Hence, $\tilde{A} \in E(\tilde{N},m)$ if and only if for all $a$, $a_{T-1} < a \leqslant a_T$ $s(a; \tilde{N},\tilde{A}) < m$.

Q.E.D

*Proof of Theorem.* The proof is by induction on $m$. For $n = 1$ we must have $m = 1$ and, clearly $\{q(1)\} \in E(\{1\},1)$. Assume that the theorem holds for any society with no more than $n$ individuals. We shall show that it also holds for a society $\tilde{N} = \{1,2, \ldots, n + 1\}$ with $n + 1$ individuals. If $m = n + 1$, then by Result 3, every $\alpha$, $q(n + 1 - m + 1) = q(1) \leqslant \alpha \leqslant q(n + 1) = q(m)$ satisfies $\{\alpha\} \in E(\tilde{N},m)$. Thus, the theorem holds.

Suppose, therefore, that $m \leqslant n$ and denote $N = \{1,2, \ldots, n\}$ (i.e., $N = \tilde{N}/\{n + 1\}$). If $q(n - m + 1) \leqslant q(m)$, then by Result 3 there exists $\{\alpha\} \in E(N,m)$ and Result 4 yields the theorem for this particular case.

It remains, therefore, only to consider the case where $m \leqslant n$ and $q(m) < q(n - m + 1)$. Then, the induction hypothesis implies that: $E(N,m) \neq \phi$, every $m$-equilibrium for $N$ contains at least two alternatives, and there is an $m$-equilibrium that contains both $q(m)$ and $q(n - m + 1)$. Choose $A^* = \{\alpha_1, \ldots, \alpha_T\} \in E(N,m)$ to be such that $\alpha_1 = q(m)$, $\alpha_T = q(n - m + 1)$ and every $\{\gamma_1, \ldots, \gamma_k\} \in E(N,m)$, with $\gamma_1 = q(m)$, $\gamma_k = q(n - m + 1)$ satisfies $\gamma_{k-1} \leqslant \alpha_{T-1}$. (Such an $A^*$ exists because the set of alternatives, $\Omega$, and hence $E(N,m)$, are finite sets). Define $\tilde{A} = \{\alpha_1, \ldots, \alpha_{T-1}\}$, where $\tilde{\alpha}_{\mathrm{T}} \equiv q(n - m + 2)$. If $\tilde{A} \in E(\tilde{N},m)$, we are done. Otherwise, by Lemma 1, the set $Z = \{b \in \Omega | b \leqslant \alpha_T \text{ and } s(b; N,\tilde{A}) \geqslant m\}$ is nonempty. Define $\beta^* = \mathrm{Max}\ \{b | b \in Z\}$. (Such a $\beta^*$ exists since $\Omega$, and hence $Z$, are finite sets.) Clearly, $q(m) \leqslant \alpha_{T-1} < \beta^* \leqslant \alpha_T = q(n - m + 1)$. Denote $P = \{i \in \tilde{N} | q(i) < \beta^*\}$, and let $M$ be a set of $m$ identical individuals whose preferences are single peaked at $\beta^*$. (Note that it is possible that the peak of no individual in $N$ is $\beta^*$, i.e., that $M \cap N = \phi$.) Since $\beta^* \leqslant \alpha_T = q(n - m + 1)$, $P \cap \{n - m + 1, \ldots, n + 1\} = \phi$. Thus the cardinality of $P$ does not exceed $n - m$. Hence, the cardinality of $N' \equiv P \cup M$ does not exceed $n$. Moreover, since $\beta^*$ (the peak of the last $m$ individuals of the society $N'$), satisfies $\beta^* > \alpha_{T-1} \geqslant q(m)$, by the induction hypothesis there exists $B^* = \{\beta_1, \ldots, \beta_H\} \in E(N',m)$ with $\beta_1 = \alpha_1 = q(m)$ and $\beta_H = \beta^*$. We shall conclude the proof of the theorem by proving:

PROPOSITION. $\tilde{B} \equiv B^* \cup \{\tilde{\alpha}_T\} = \{\beta_1, \ldots, \beta_{H-1}, \beta_H, \tilde{\alpha}_T\} \in E(\tilde{N},m)$.

In order to establish the proposition we need the following:

LEMMA 2. $\beta_{H-1} \leqslant \alpha_{T-1}$.

*Proof.* We shall show that if $\beta_{H-1} > \alpha_{T-1}$, then $C \equiv \{\beta_1, \ldots, \beta_{H-1},\alpha_T\} \in E(N,m)$, which contradicts the choice of $A^* = \{\alpha_1, \ldots, \alpha_{T-1},\alpha_H\} \in E(N,m)$. (Recall that if $\{\gamma_1, \ldots, \gamma_k\} \in E(N,m)$ with $\gamma_1 = q(m)$ and $\gamma_k = q(n - m + 1)$, then $\gamma_{k-1} \leqslant \alpha_{T-1}$.)

Assume, therefore, that $\beta_{H-1} > \alpha_{T-1}$. Then,

(i) For all $b < \beta_{H-1}$, $s(b; N,C) \geqslant m$ if and only if $b \in C$: Indeed, by Observation 3, for all $b < \beta_{H-1}$, $s(b; N,C) = s(b; P,B^*) = s(b; N',B^*)$. Since $B^* \in E(N',m)$, Definition 3 yields (1).

(ii) $s(\beta_{H-1}; N,C) \geqslant m$: Indeed, since $\beta_{H-1} < \beta_H$

$= \beta^* \leqslant \alpha_T$, we have from observation 6 that $S(\beta_{H-1}; N,C) \supset S(\beta_{H-1}; N,B^*) = S(\beta_{H-1}; P,B^*) = S(\beta_{H-1}; N'B^*)$. Since $B^* \epsilon E(N'm)$, Definition 3 yields (ii).

(iii) For all $b > \beta_{H-1}$, $s(b; N,C) \geqslant m$ if and only if $b \epsilon C$; that is, $b = \alpha_T$: Indeed, since we assumed that $\beta_{H-1} > \alpha_{T-1}$, Observation 6 and Definition 2 imply that $s(b{:}\ N,C) \leqslant s(b; N,A^*) < m$ for all $b > \beta_{H-1}$, $b \neq \alpha_T$. Moreover, Observation 4 implies that $s(\alpha_T; N,C) \geqslant m$.

Thus, (i), (ii), (iii), and Definition 3 imply that $C \epsilon E(N,m)$, which concludes the proof of Lemma 2.

Q.E.D.

### Proof of the Proposition

(iv) For all $b < \beta^* = \beta_H$, $S(b; \bar{N},\bar{B}) \geqslant m$ if and only if $b \epsilon \bar{B}$: Indeed, by Observation 3 for all $b < \beta^*$, $S(b; \bar{N},\bar{B}) = S(b; P,B^*) = S(b; N',B^*)$. Since $B^* \epsilon E(N',m)$, Definition 3 yields (iv).

(v) $s(\beta_H; \bar{N},\bar{B}) \geqslant m$: Indeed, since by Lemma 2, $\beta_{H-1} \leqslant \alpha_{T-1}$ then Observation 6 implies $S(\beta_H; \bar{N},\bar{B}) \supseteq S(\beta_H; \bar{N},\bar{A})$. But $\beta_H = \beta^* \epsilon Z$ hence $s(\beta_H; \bar{N},\bar{A}) \geqslant m$.

(vi) For all $b$, $\beta^* = \beta_H < b \leqslant \alpha_T$, $s(b; \bar{N},\bar{B}) < m$: Indeed, since $\beta^* > \alpha_{T-1}$, by Observation 6, $S(b; \bar{N},\bar{B}) \subset S(b; \bar{N},\bar{A})$ for all $b$ with $\beta^* < b \leqslant \alpha_T$. But, by the choice of $\beta^*$, $s(b; \tilde{N},\tilde{A}) < m$ for all $b$ with $\beta^* < b \leqslant \alpha_T$.

(vii) For all $b$, $\alpha_T < b < \bar{\alpha}_T$, $s(b; \bar{N},\bar{B}) \leq m$: Indeed, by Observation 1 $S(b; \bar{N},\bar{B}) \subset \{1, \ldots, n - m + 1\}$ for all $b < \bar{\alpha}_T$. Therefore, by Observation 5 $S(b; \bar{N},\bar{B}) \subset S(\alpha_T; \bar{N},\bar{B})$ for all $b$, $\alpha_T < b < \bar{\alpha}_T$. Thus, (vi) yields (vii).

(viii) For all $b$, $b \geqslant \bar{\alpha}_T$, $s(b; \bar{N},\bar{B}) \geqslant m$ if and only if $b \epsilon \bar{B}$, that is, $b = \bar{\alpha}_T$; follows directly from observations 4 and 2.

By Definition 3, (iv)-(viii) imply that $\bar{B} \epsilon E(\bar{N},m)$ which completes the proof of the proposition and hence of the theorem.

Q.E.D.

Now, Result 2 is, of course, an immediate corollary of the theorem. Moreover, if $q(m) < q(n - m + 1)$, then by Result 3, there is no $m$-equilibrium which consists of a single alternative, and the theorem yields Result 5.

*Proof of Result 6.* Because by Result 2, $E(N,m) \neq \phi$ and clearly, $E_c(N,m) \subset E(N,m)$, we have only to prove that for any $A = \{a_2, \ldots, a_T\} \epsilon E(N,m)$, $A$ is a consecutive $m$-equilibrium, that is, that $S(a_t; N,A)$ is consecutive for each $a_t \epsilon A$. Let $A = \{a_1, \ldots, a_T\} \epsilon E(N,m)$, i,j,k $\epsilon N$ with $i < j < k$ and suppose that for some $\tau$, $\{i,k\} \subset S(a_\tau; N,A)$. We have to show that $j \epsilon S(a_\tau; N,A)$.

Since $\{i,k,\} \subset S(a_\tau; N,A)$, then for all $a_t \epsilon A$, $t \neq \tau$

$$|q(i) - a_t| > |q(i) - a_\tau| \text{ and}$$
$$|q(k) - a_t| > |q(k) - a_\tau|.$$

Therefore, for all $q$, $q(i) \leqslant q \leqslant q(k)$, for all $t \neq \tau$

$$|q - a_t| > |q - a_\tau|.$$

In particular, this inequality holds for $q = q(j)$, that is, $j \epsilon S(a_\tau; N,A)$.

Q.E.D.

### Proofs of Results 7 and 8

*Proof of Result 7.* Consider the society $N = \{1,2, \ldots, 10\}$, with 10 voters, in which the set of alternatives consists of 10 alternatives given by $\Omega = \{a_1,a_2, \ldots, a_{10}\}$, where $a_1 < a_2 < \ldots < a_{10}$.

All that is required of the preference ordering, $\geqslant_i$, of individual $i \epsilon N$ over $\Omega$ is that it be single peaked, with $i$'s peak being $a_i$, and also satisfy the following conditions.

For $i = 4$, $a_4 \succ_4 a_5 \succ_4 a_6 \succ_4 a_3$ and $a_3 \succ_4 a_j$ for all $j \notin \{3,4,5,6\}$; for $i = 5$, $a_5 \succ_5 a_6 \succ_5 a_4$ and $a_4 \succ_5 a_j$ for all $j \notin \{4,5,6\}$; for $i = 6$, $a_6 \succ_6 a_5 \succ_6 a_7$ and $a_7 \succ_6 a_j$ for all $j \notin \{5,6,7\}$; for $i = 7$, $a_7 \succ_7 a_6 \succ_7 a_8$, and $a_8 \succ_7 a_j$ for all $j \notin \{6,7,8\}$. Finally, let the quota $m$ be equal to 3. Then, it is easy to check that $A = \{a_3,a_5,a_8\}$ constitutes a 3-equilibrium, where $S(a_3; N,A) = \{1,2,3\}$; $S(a_5; N,A) = \{4,5,6\}$; $S(a_8; N,A) = \{7,8,9,10\}$.

Suppose for simplicity that when an elected candidate deviates from an equilibrium, the resulting new equilibrium that he induces is the one in which no other elected candidate deviates and also, that his move does not enable a new potential candidate to receive $m$ or more votes. Then, clearly the elected candidates in $A$ that offer $a_3$ and $a_8$ will not deviate. Now, if the second elected candidate will offer alternative $a_6$ instead of $a_5$ (which is the one he is currently offering), the resulting induced equilibrium will be $B = \{a_3, a_6, a_8\}$, where $S(a_3; N,B) = \{1,2,3\}$; $S(a_6; N,B) = \{4,5,6,7\}$; $S(a_8; N,B) = \{8,9,10\}$.

It is easy to verify that $B$ is a stable equilibrium, and therefore, no elected candidate in $B$ has an incentive to deviate from it. Thus, the only equilibrium that the second elected candidate in $A$ can generate via $a_6$ is $B$. But in $B$, the number of individuals who vote for him (= 4) is greater than the number who vote for him in $A$ (= 3). Therefore, the second elected candidate has an incentive to deviate from $A$ via $a_6$. Hence $A$ is not a stable 3-equilibrium.

Q.E.D.

In order to prove Result 8 we first need:

LEMMA 3. Let $A = \{a_1, \ldots, a_T\}$ be an $m$-equilibrium. If an elected candidate $\tau$ has an incentive to deviate from $A$ via $w$, then $\tau$ *cannot* generate $A$ from $A$ via $w$.

*Proof.* Recall that $\tau$ has an incentive to deviate from $A$ via $w$, if and only if by offering $w$ instead of $a_\tau$, he can guarantee himself a support with at least $\alpha \equiv s(a_\tau; N,A) + 1$ voters in any equilibrium that will result. But this means that whoever offers $a_\tau$ in $A$ can guarantee himself (by offering $w$ instead of $a_\tau$) at least $\alpha$ voters. Now, assume in negation, that $\tau$ could generate $A$ from $A$ via $w$. Then, when the generated $A$ obtains, the candidate who is now offering $a_\tau$ can also guarantee himself $\alpha$ votes and again "regenerate" $A$, at which stage yet another candidate (who is the one offering $a_\tau$ in this second iteration) will deviate and guarantee himself a support of size no less than $\alpha$ after which, once again, $A$ will be generated and so on. After regenerating $A$ $n$ times, we will have that $n$ different candidates in (the $n$th iteration of) $A$ have a support of at least $\alpha$ individuals. But this is, of course, impossible, since then, the total number of supporters of candidates is $A$ is at least $n\alpha \geqslant n(M + 1) > n$. CONTRADICTION.

Q.E.D.

*Proof of Result 8.* Let $m \leqslant n$ be given. Then by Result 2, there exists $A_0 = \{a_1, \ldots, a_T\} \epsilon E(N,m)$. If $A_0$ is stable, we are done. Otherwise, there exists an elected candidate $\tau_1$ and an alternative $w_1 \epsilon \Omega$ such that $\tau_1$ has an incentive to deviate from $A_0$ via $w_1$. In particular, there exists $A_1 \epsilon E(N,m)$ which is induced by $\tau_1$ from $A_0$ via $w_1$. If $A_1$ is stable, we are done. Otherwise, there is an elected candidate $\tau_2$ (who may be the same as $\tau_1$) and an alternative $w_2 \epsilon \Omega$ such that $\tau_2$ has an incentive to deviate from $A_1$ via $w_2$, thereby inducing $A_2 \epsilon E(N,m)$. Continuing this process yields the sequence $\mathbf{A} = \{A_0, A_1, A_2, \ldots\}$. We claim that $\mathbf{A}$ is finite, and hence there exists a stable $m$-equilibrium. Suppose to the contrary. Then, since the set of all $m$-equilibria is finite, if $A$ is infinite there exist two different natural numbers, $k$ and $l$, such that $A_k = A_l$ and $\{A_k A_{k+1}, \ldots, A_l\} \subset \mathbf{A}$. But then the candidate who induces $A_{k+1}$ from $A_k$ also generates $A_l$ from $A_k$, contradicting Lemma 3. Thus, $\mathbf{A}$ is finite, and by construction, the last element of $\mathbf{A}$ is a stable $m$-equilibrium.

Q.E.D.

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# Risk and Uncertainty as Factors in the Durability of Political Coalitions

JOHN R. WRIGHT
*University of Iowa*

ARTHUR S. GOLDBERG
*University of Rochester*

*The central purpose of this article is to shed some light on the roles that risk attitudes and uncertainty may play in determining the durability of political coalitions. Although Axelrod theorized that polarized, or "unconnected," coalitions would be less durable than "connected" coalitions, subsequent empirical research has found little support for Axelrod's hypothesis. In this article we demonstrate theoretically that many of the anomalies associated with Axelrod's polarization hypothesis can be explained by incorporating risk attitudes and uncertainty into a logical model of a coalition maintenance process. The model we have constructed assumes that coalition members are uncertain about their ultimate payoffs and therefore are subject to persuasion and manipulation by a coalition leader. Through computer simulation we demonstrate that polarized coalitions are indeed capable of enduring for relatively long periods of time once members' risk characteristics and uncertainty are taken into consideration.*

An essential element of successful political leadership is the ability to maintain a supporting coalition. Yet surprisingly little is known about the ways in which political leaders keep their coalitions intact, and little is known about the properties of coalitions that most affect their longevity. Generally, political scientists have been much more interested in questions about the formation of coalitions than in questions about the maintenance of coalitions. But theories that are useful for explaining which coalitions form often are of little use for explaining which coalitions are most durable.[1] To the extent that formation and maintenance entail different processes, additional research on the maintenance of coalitions is clearly needed.

The objective of this research is to formalize a dynamic model of a maintenance process and, through computer simulation, to assess the impact on longevity of three properties of coalitions and their members. The three properties examined are: polarization, that is, the divergence in the long-term policy goals of the coalition members; uncertainty, the members' subjective estimates of receiving their preferred payoffs from a coalition; and risk attitudes, the willingness of members to gamble on receiving their preferred outcomes from a coalition.

Of these three variables, only polarization has been the subject of previous investigation. Although a substantial portion of the literature of coalition theory treats actors as maximizers of expected utility (e.g., De Swaan, 1973; Leiserson, 1966; and Riker, 1962), that same body of literature speaks hardly at all to the roles of risk characteristics and uncertainty in the decision processes of those actors.[2] The work described herein is an risk attitudes and uncertainty into a dynamic model, we seek not only to provide directions for future research, but also to provide a systematic way of conceptualizing the process, or art, of political leadership.

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[1] For example, minimal winning status is an important theoretical factor in the formation of coalitions, but there is considerable variance in the durability of minimal winning coalitions, once formed. See Dodd (1976, p. 141).

[2] One notable exception is the work of Bueno de Mesquita (1975, 1981).

## Research on the Durability of Coalitions

Virtually all theoretical and empirical work on coalition durability since Axelrod (1970, p. 167) has included some notion of policy polarization, or divergence in the policy preferences of coalition members. Axelrod proposed that coalitions with low conflict of interest—as measured by the spatial distances among the policy positions of coalition members—"can be expected to last longer once formed than the average coalition, just because disputes within such a coalition will be easier to resolve." Because Axelrod was not able to measure precisely the spatial distances among parties along some ideological continuum, he introduced the notion of a "connected" coalition in order to capture the amount of dispersion in the policy positions of coalition parties. A connected coalition was defined as a coalition consisting of adjacent parties.

Axelrod's hypothesis was nicely borne out by his own data on the Italian parliament from 1953 through 1969. Data from other European parliaments, however, have proved to be much less supportive. Neither Laver (1974) nor Warwick (1979) found statistically significant evidence that connected coalitions, as defined by Axelrod, lasted longer than unconnected coalitions, and Dodd (1976), using a variable that took into account both the number of conflict dimensions and the dispersion of parties along those dimensions, concluded that polarization had very little effect on cabinet durability.

In part, the lack of strong empirical support for Axelrod's theory can be attributed to differences (and difficulties) in operationalizing the concept of polarization. But regardless of the measurements used, support for the polarization hypothesis is generally weak. Evidence indicates that polarized coalitions frequently do endure, and specific examples are not hard to find. Among relatively polarized coalitions, the Zoli government in Italy lasted for 13 months—just over the average for the duration of all coalitions in the data presented by Axelrod (p. 178). The Bidault government of 1949, in France, lasted for nine months, a fair run for a Fourth Republic government (De Swaan, 1973, p. 184). Finally, in Israel, the Eshkal government of 1966 governed for 16 months (De Swaan, p. 234). All of these were unconnected, or "open," coalitions. To be sure, they were not among the most long-lived of coalitions, but neither did they vanish in the blink of an eye.

The inability of the polarization hypothesis to account for such anomalies has encouraged a search for new explanations of coalition durability. Dodd (1976), for example, argues that durability depends on the size of cabinet coalitions, and that the size of cabinet coalitions depends on a complex interaction of variables in the parliamentary party system: the number of parties, the relative strength of parties, the variance in party positions, and the number of salient ideological cleavages.

Although Dodd's empirical results are generally consistent with his theory, his model nevertheless fails to account for most of the variance in the durability of coalitions. His parliamentary party system variables account for only 20% of the variance in durability, and the size of the coalition (i.e., the degree to which the coalition approaches minimum winning status) also explains only about 20%. At best, then, 60% of the variance in the durability of coalitions remains to be accounted for.

The extensive unexplained variance in models of cabinet durability reveals the limitations of existing research strategies. Hence, Dodd (1984, p. 155), for one, has suggested that the challenge for scholars is to "reassess the conceptual and methodological underpinnings of current work and to explore new empirical avenues for research." One alternative research strategy is to move away from strictly deterministic models and to begin to examine the stochastic components of coalitional maintenance situations (e.g., Cioffi-Revilla, 1984). In the approach taken here, we too introduce a probabilistic element into the maintenance process. We assume that coalition actors practice some Bayes-like probability estimation procedures, so that durability depends not just on party system characteristics, but also on the uncertainty and risk-raking characteristics of the actors.

## Theoretical Considerations

### Uncertainty as a Central Variable

The bearing of uncertainty upon the durability of coalitions derives from the fact that both leaders and members of coalitions depend upon their perceptions of one another to estimate each other's future behaviors. Insofar as they entail likely future policy behaviors, these perceptions are not likely to consist of point estimates held with absolute certainty. Rather, subjective beliefs about another's behavior may reasonably be characterized as probability distributions (e.g., DeGroot, 1970, chap. 6), and the confidence or lack of confidence one has about someone else's future behavior may be described in terms of the variance in those distributions. The greater the variance, the less confident one would be about another's likely behavior.

This is not to say that real political actors think explicitly in terms of probability distributions and variances. Instead, their behaviors suggest that these may be reasonable terms in which to describe their decision making under uncertainty. Thus, if one had only a brief history of experience with a particular individual (i.e., only a few data points on which to base a judgment) whose behavior has been quite normal, one is apt to give more weight to a single instance of aberrant behavior (e.g., rage) than if one had a long experience of the person behaving calmly (i.e., many data points, very few of which involve rage). Similarly, with regard to the anticipated behaviors of two individuals with each of whom one has had a brief acquaintance, one is apt to be more confident about the future behavior of one than of the other, if that one—even on brief acquaintance—has behaved more consistently than the other.

Just as with these everyday experiences, the durability of political coalitions ought to be affected substantially by variances in the probability distributions each member associates with the likely long-run behaviors of the other members. Consider the situation from one possible perspective of the leader of party *A*. Let us call that person *L*, and let us assume that in terms of mean estimates, *L* perceives the relative positions of all other parties as depicted in Figure 1. However, let us also assume that *L* is not certain of the position of any of the parties except Party *A*, and that his degree of uncertainty varies from party to party, as represented by the probability distributions in Figure 2.

*L*'s mean estimates of the position of each of the parties, as indicated by the vertical dashed lines in Figure 2, is the same as in Figure 1. However, the probability distributions that *L* associates with those same means open up an interesting possibility. Imagine a utility function for *L* such that association with policy positions to the right of point *X* have an unacceptable cost—perhaps loss of the leadership position or even assassination. Then, consider the wide variance about *L*'s estimate of party *C*'s position. As compared with *L*'s perception of party *D*'s position, the probability that party *C* will adopt a position to the right of *X* is quite high. Thus, one could reasonably expect *L* to prefer the coalition *ABD* rather than *ABC*, because the probability of being associated with a policy position to the right of *X* is considerably smaller with coalition *ABD*.

Coalition *ABD*, however, is an "open" coalition in Axelrod's terminology and therefore should be less durable than coalition *ABC*, a "connected" coalition. But if we assume that *L* would withdraw from either coalition if his mean estimate of one of the partner's positions fell to the right of *X*, coalition *ABD* is likely to retain *L*'s allegiance longer than coalition *ABC*. Based upon the distributions in Figure 2, it would take relatively few instances of movement to the right on the part of *C* to move *L*'s mean estimate of *C*'s position to the right of *X*. But because of the rather spiked distribution that *L* has with regard to *D*'s position, it would take considerably more instances of rightward movement on the part of *D* to move *L*'s mean estimate of *D*'s position to the right of *X*. Thus, *ceteris paribus*, the coalition *ABD* might well be more durable than coalition *ABC*.

## Risk Taking as a Central Variable

Under the maxims of utility theory, the attitude of an individual toward risk on a given issue is captured in the shape of his utility function regarding that issue. An individual is regarded as risk averse on a given issue if his utility function with respect to that issue is concave in relation to the horizontal axis, and as risk acceptant if that function is convex in relation to the horizontal axis, because of the theoretical implications of these shapes for the relationship between a particular outcome with certainty, on the one hand, and a gamble between best and worst outcomes, on the other (Friedman & Savage, 1948).

The bearing of risk characteristics on the durability of coalitions may be demonstrated by a discussion of the relationships presented in Figure 3. Therein, alternate utility functions are presented for some member *M* of coalition *C* for policy outcomes $O_b$, $O_w$, and $O^*$. Let $O^*$ be the outcome *M* is certain of achieving by not being a member of coalition *C* (i.e., *M*'s assured payoff from an alternative coalition). Let outcome $O_b$ be the best outcome *M* could realize by belonging to coalition *C*, and let $O_w$ be the worst outcome that *M* could

**Figure 1. Party Positions on a Policy Continuum**

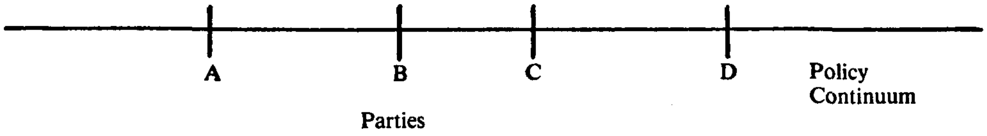
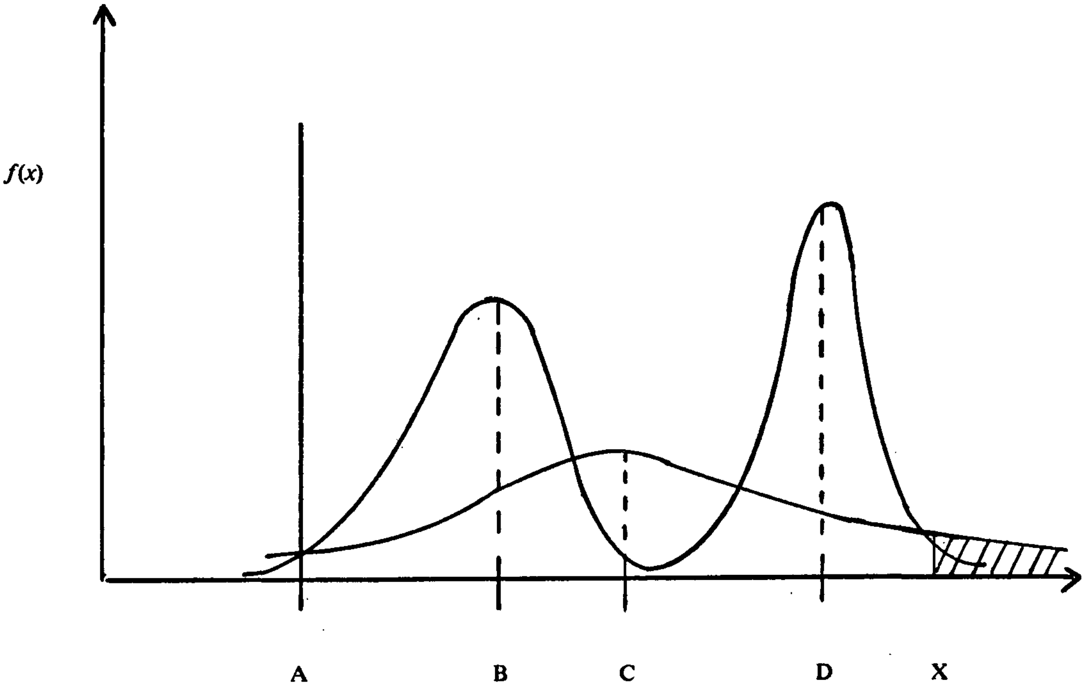


Figure 2. L's Perceptions of Party Positions



realize by being a member of $C$. Associated with $O_w$ is a utility value of -1, and associated with $O_b$ is a utility value of +1. This is true regardless of the shape of $M$'s utility curve. However, this is not the case for $O^*$. If $M$ is a risk taker, then the utility associated with $O^*$ is -.5; and if $M$ is a risk averter, the utility associated with $O^*$ is +.5.

The decision problem for $M$ is to choose between two alternative courses of action: to remain in coalition $C$ or to leave coalition $C$. Assuming $M$ is uncertain about the payoff he will receive from coalition $C$, and assuming $M$ is a rational utility maximizer, $M$ will choose that action which yields him the largest expected utility. To allow $M$ to calculate expected utilities, we assign subjective probabilities to outcomes $O_w$ and $O_b$. For purposes of illustration in Figure 3, we assume that $M$ anticipates $O_w$ and $O_b$ each with probability one-half.

Because $O_w$ and $O_b$ are equally likely, $M$'s expected utility from staying in the coalition is $(.5)(1)+(.5)(-1)=0$. Alternatively, because $M$ is assured of obtaining $O^*$ if he leaves the coalition, his utility from leaving is -.5 or .5, depending on whether he is a risk taker or a risk averter. Suppose $M$ is a risk taker. Then, being a rational utility maximizer, $M$ will prefer to stay in coalition $C$, because the expected utility of staying (0) is greater than the utility from leaving (-.5). But just the opposite holds if $M$ is a risk averter. In that case, the utility from leaving, .5, will be greater than zero. Thus, *ceteris paribus,* the loyalty of coalition members will depend on the shape of their utility curves over the set of possible policy outcomes. Members who are risk acceptant are more likely to remain loyal to the coalition than members who are risk averse.

## The Interpretation of Risk Attitudes

The decision problem depicted in Figure 3 has several basic properties that are helpful in explicating and interpreting the concepts of risk acceptance and risk aversion. In order to develop these properties, let $P_{ij}$ be coalition member $i$'s subjective probability of obtaining outcomes $O_w$ and $O_b$ from the coalition $C$, and let $U_i^*$ be the utility associated with outcome $O^*$ for member $i$; $i=1,2$ and $j=w,b$. (We now drop the assumption that all probabilities equal one-half.) Let Member 1 have a risk-averse utility curve and Member 2 a risk-acceptant utility curve. (Assume $U_1^*$ is greater than $U_2^*$.) Then, Member 1 will remain in the coalition only if $P_{1b}-P_{1w} >= U_i^*$, whereas Member 2 will remain in the coalition only if

$P_{2b}-P_{2w} > = U_2^*$. For just two outcomes, $P_{iw}=1-P_{ib}$; and the minimum value of $P_{1b}$ that will keep Member 1 in the coalition is:

$$P_{1b}^* = U_1^* + (1-P_{1b}) = (U_1^*+1)/2. \qquad (1)$$

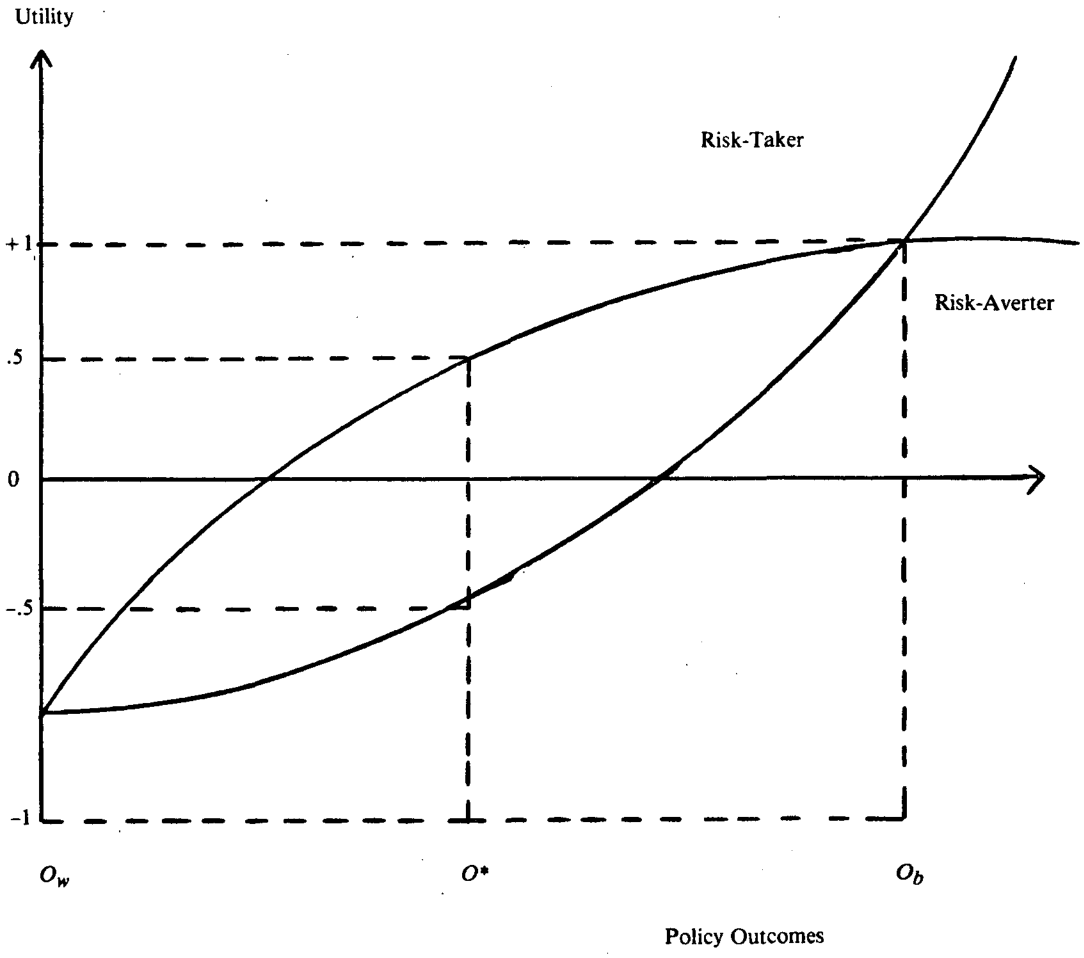
Similarly, the minimum value of $P_{2b}$ that will keep Member 2 in the coalition is:

$$P_{2b}^* = U_2^* + (1-P_{2b}) = (U_2^*+1)/2. \qquad (2)$$

Therefore, if coalition members are rational, they will remain in the coalition only if their subjective probabilities of obtaining their best outcomes are at least as large as the minimum values, $P_{ib}^*$, that satisfy equations (1) and (2). Very literally, these minima can be thought of as the members' *security levels.* They indicate the level of assurance, or probability, that each member must have of achieving his preferred payoff. Any time a follower's subjective probability is at least as large as his security level, the follower believes he is in the best coalition possible. But any time his beliefs change such that the probability of receiving the preferred payoff drops below the security threshold, the rational member will leave the coalition.

From equations (1) and (2) it follows that different security levels, that is, different values of $P_{ib}^*$, reflect different degrees of convexity or concavity in utility curves. Since $U_1^* > U_2^*$, $P_{1b}^*$ must be greater than $P_{2b}^*$. Consequently, individuals with risk-averse utility curves will require higher probabilities of achieving their preferred outcome than individuals with risk-acceptant utility curves,

**Figure 3. Alternate Utility Functions for Coalition Member M**



other things being equal. Security levels, then, are indicators of risk attitudes and will be treated as such in the ensuing analysis.

Security levels, like other attitudinal characteristics, are determined to some extent by factors totally outside of the model (e.g., genetics and social conditioning). However, security levels are also determined by the external coalitional opportunities available to a player. Clearly, the better $O^*$, the higher utility one can be certain of by belonging to some other coalition besides $C$. As indicated by equations (1) and (2), higher utilities for $O^*$ will yield higher values of $P^*_{1b}$ and $P^*_{2b}$. This is perfectly reasonable in that the better one can be assured of doing outside of coalition $C$, the greater assurance one must have that $C$ will provide the desired payoff. Thus, the more attractive the external coalition opportunities are to member $M$ of coalition $C$, the higher will be $M$'s security level.

The theoretical considerations presented thus far suggest that attitudes toward risk taking and uncertainty do indeed have a bearing on the longevity of political coalitions. This theoretical discourse, however, does not, by itself, yield insights into the extent to which security levels and uncertainty affect the longevity of coalitions; nor does it yield insights into the dynamics of the maintenance process. In order to develop the dynamic properties of the model as well as to test the model's sensitivity to risk attitudes and uncertainty, we have developed a computer algorithm that simulates the roles of risk, uncertainty, and polarization in the maintenance process. We turn now to the formal development of that algorithm.

## A Model of a Coalition Maintenance Process

Our model of a maintenance process is based on a three-person coalition: two followers, or members, and a leader.[3] We let $N=\{1,2\}$ denote the set of followers and let $L$ be the leader.[4] $L$ is given complete control over payoffs to $N$, and these payoffs are assumed to take the form of policy outcomes. $L$ is assumed to make a series of policy decisions, each decision occurring at some point in time, $t$. Moreover, these policy decisions, or allocations, are assumed to occur at regular intervals, so that an allocation at $t=6$, for example, is three times as far into the future as an allocation at $t=2$.

We let $Z$ be a discrete random variable that assigns a real number, either 1 or 0, to each of $L$'s policy decisions. Each allocation, therefore, is dichotomous in nature, such as a decision to spend money on guns or on butter. The number of such allocations $L$ is allowed to make is unrestricted, so that in effect one could think of $L$ as sampling with replacement from an urn of 1s and 0s, where each draw represents a policy outcome.

The members of the coalition are required to have opposite preferences. Follower 1 always prefers 1 to 0 and Follower 2 prefers 0 to 1. In addition, we assume that for each follower there is some fixed proportion of 1s or 0s that represents his minimum acceptable payoff (*MAP*). The *MAP* represents the smallest payoff the follower would accept with certainty in exchange for participation in the coalition. If one thinks in terms of sampling from an urn, each follower's *MAP* would be the minimum (or maximum) proportion of 1s he would be willing to accept if the urn of 1s and 0s were divided between the followers at a single point in time. The difference in the followers' minimum (or maximum) acceptable proportion of 1s indicates the degree of polarization in the model coalition.

Each follower is assumed to require his minimum acceptable payoff with some fixed probability. Follower 1 might, for example, require 60% 1s with probability .75, and Follower 2 might require 60% 0s with probability .80. These fixed probabilities are the members' security levels, and they remain fixed throughout the sequence of allocations.

## The Followers' Decision Problem

The decision problem for the followers is to estimate the likelihood of receiving their *MAP*s. This is problematic for the followers because we assume that they are not certain about $L$'s "true" policy position—that is, the proportion of 1s that $L$ ultimately will allocate. As a result, the followers must make probabilistic estimates of receiving their *MAP*s, and in order to allow them to do so, we assume that each follower has a subjective probability distribution over the set of $L$'s possible policy positions. Figure 4 illustrates one possible distribution for a follower whose *MAP* is 60% 1s. The shaded area is the follower's subjective probability estimate of receiving his *MAP*.

After each allocation by $L$, we assume that the followers update their subjective probability estimates. Thus, the followers' distributions will shift

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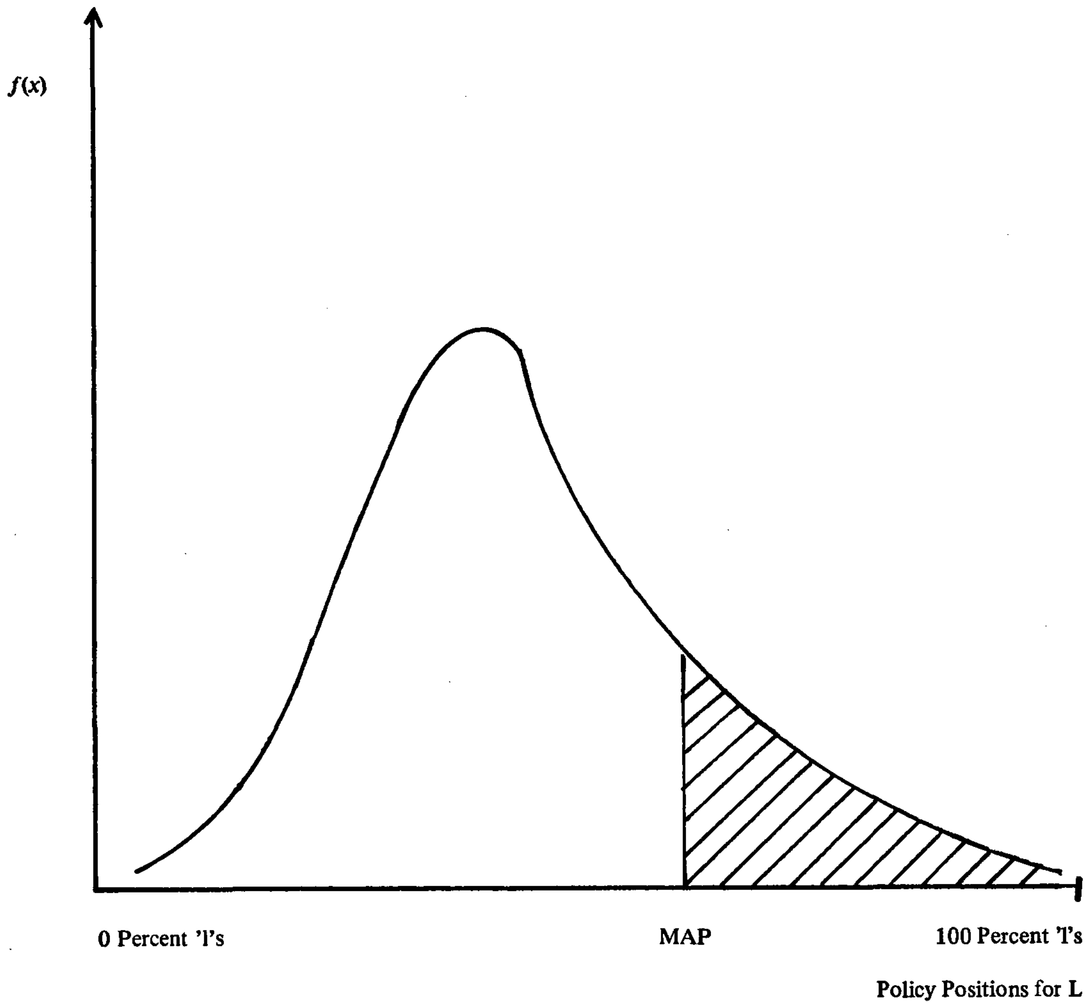
[3] The model is based on just a three-person coalition in order to keep the analysis simple. Even so, a number of important real-world situations can be described in terms of three-person coalitions—for example, the tripartite governments of the early French Fourth Republic.

[4] One might think of the followers as leaders also, but as leaders of political parties. The leader, $L$, however, is always the leader of a coalition of parties.

**Figure 4. Coalition Member's Subjective Probability Distribution**



one way or the other depending on the allocations they observe. In Figure 4, for example, the more 1s the follower observes, the larger will be the area under the curve to the right of the *MAP* point.

A formal prescription for updating a subjective probability estimate on the basis of prior information and sequential observations is given by Bayes' Theorem. Under a Bayesian process, an a priori distribution at time 1 becomes an a posteriori distribution at time 2, which becomes a new a priori distribution at time 3, and so forth. In problems of decision making under uncertainty, any decision rule that prescribes an action that maximizes one's expected utility given some a priori distribution is said to be a *Bayes decision rule* against the a priori distribution. (See Luce & Raiffa, 1957, pp. 309-313.) In the present context, we assume that the followers in the coalition are rational Bayesian actors in the sense that each employs a Bayes decision rule.

The Bayes decision rule, or the optimal decision rule, for the followers in the present context is to leave the coalition any time their probability estimates drop below their security levels. In Figure 4, for example, a follower who preferred 1s to 0s would compare his security level with the value of the area of the shaded region, and if the shaded area were smaller than his security level, he would leave the coalition.

The formalization of the followers' decision rule requires the following definitions.

DEFINITION 1. Let $\Theta$ be an allocation rule by which $L$ chooses some $z \in \{0,1\}$ at each decision point. $L$'s decision at time $t$ is given by $\Theta(t) = z$.

Definition 2. Let a series of decisions by $L$ be called a sequence, and let a sequence of $t$ elements be denoted by $S_t$. As an example, one possible sequence of three elements would be denoted by $S_3 = (0,1,0)$.

Definition 3. Let $r$ be the number of 1s in $t$ allocations.

Definition 4. Let $X = \{x: 0 <= x <= 1\}$, be the set of possible policy positions for $L$; where each $x \epsilon X$ represents some proportion of 1s.

Definition 5. Let $\mu_1$ be the minimum proportion of 1s acceptable to Follower 1, and let $\mu_2$ be the maximum proportion of 1s (or minimum proportion of 0s) acceptable to Follower 2.

For some fixed $t$ and $r$, let $f_i(x; r,t)$ be a subjective probability function for member $i$ over $X$ corresponding to the beta distribution.[5]

Definition 6. $f_i(x; r,t) = (1/k)x^{r-1}(1-x)^{t-r-1}$, where $r = \sum_{k=1}^{t} z_k$, and $(1/k) = (t-1)!/(r-1)!(t-r-1)!$.

The mean of the beta distribution is given by $r/t$, and the variance by $r(t-r)/t^2(t+1)$. Thus, the followers' beliefs about $L$'s position at the beginning of the sequence can be specified by adjusting the variance and the mean of their subjective probability distributions through the parameters $r$ and $t$.[6]

Definition 7. Denote $L$'s true policy position by $x^*$, where $x^* \epsilon X$ such that $\lim_{t \to \infty} Pr(|r/t - x^*| > \epsilon) = 0$ where $\epsilon$ is a small, positive constant.

In other words, $x^*$ is the point about which the followers' probability distributions eventually collapse. Hence, with enough observations, the followers become certain of $L$'s actual policy position.

Given the probability density function of Definition 6, we can now define the subjective probability estimate for follower $i$ at time $t$ that $L$'s policy position is equal to or greater than the follower's minimum or (maximum) acceptable proportion of 1s.

Definition 8.

$$P_{it} = Pr(X > \mu_i) = \int_{\mu_i}^{1} f_i(x; r,t)dx.$$

Definition 9. Let $P_1^*$ be Follower 1's security level, and let $P_2^*$ be Follower 2's security level.

With these last two definitions, the followers' decision rules are: For Follower 1, if $P_{1t} < P_1^*$, leave the coalition; for Follower 2, if $P_{2t} > P_2^*$, leave the coalition. With $\mu_1 > \mu_2$, we assume $P_1^* > 0$ and $P_2^* < 1$, implying that neither member will stay in the coalition with certainty.

The decision problem for the followers can be summarized briefly. Each follower is assumed to have some predetermined minimum long-term acceptable payoff, $\mu_i$, along with some required and constant probability of obtaining that payoff, $P_i^*$, called the security level. We interpret the difference in the followers' values of $\mu$ as the degree of polarization in their policy preferences, and each follower's value of $P_i^*$ is taken as a measure of risk taking. For each follower, the higher the value of $P_i^*$, the more confident he must be of achieving his required proportion of 1s or 0s, and the less willing he will be to risk staying in a coalition that might not provide $\mu_i$. In addition, each follower has some belief, $P_{it}$, or subjective probability estimate, that the leader's true policy position is such that, in the long run, the leader will deliver at least the minimum acceptable payoff. These subjective estimates of probability will change as the followers observe $L$'s behavior, and any time a follower's estimate of the probability that $L$ will deliver his minimum acceptable payoff does not meet his security level, $P_i^*$, the follower leaves the coalition.

Our view of the maintenance process is similar to that described by Grimsson (1982), who conceives the maintenance process as a series of connected games.

> Changes in environmental parameters, the emergence of new problems and tasks, the need for new policies, and continuous payoff allocations —all these keep coalition actors involved in an unbroken bargaining process. Coalition maintenance thus can be viewed as a series of connected games. . . . When a partner becomes more and more dissatisfied with the outcome of in-

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[5] The beta distribution fits our needs here because it is capable of representing a wide variety of priors, and information is incorporated easily to update the shape of the distribution. The beta distribution also has the property that if the prior distribution follows a beta distribution, then the posterior distribution will also follow a beta distribution (DeGroot, 1970, chap. 9).

[6] Even though a sequence, $S_t$, begins only when the coalition is first formed, the members of the coalition are assumed to have had different experiences with $L$ before the formation of their coalition. In other words, each follower enters the coalition with some expectation of $L$'s position. This expectation, or prior, might be based on observations of previous allocations of 1s and 0s by $L$, or on verbal commitments by $L$.

> dividual games, the maintenance propensity is likely to fall and the process of dissolution will set in. (p. 177)

### The Leader's Decision Problem

The leader's role in the present model is to make sequential allocations of 1s and 0s. What makes the leader's role significant, though, is that, we endow him not only with perfect information about his followers' preferences, uncertainty, and security levels, but also that we allow him to choose 1s and 0s *in any sequence he wishes.* Thus, the leader is given the power to manipulate the followers' beliefs of receiving their minimum acceptable payoffs. By allocating a series of 1s, for example, the leader could increase a follower's confidence of receiving his *MAP* (provided the follower preferred 1s to 0s) and, therefore, strengthen his allegiance to the coalition.

We assume that the leader's sole objective is to maximize the longevity of his coalition and therefore the longevity of his leadership. The decision problem for the leader, then, is how to allocate a sequence of 1s and 0s that is optimal in the sense that it keeps the coalition together for as long as possible. In order to accomplish this objective, the leader needs an optimal allocation decision rule, and we turn now to the derivation of such a rule.

Thus far only a particular sequence, $S_t$, consisting of any number of 1s and 0s has been defined. In order to identify an optimal allocation rule for *L,* we now define a set of sequences, $\bar{S}$, the set of *uninterrupted* sequences, which have the property that both members are still in the coalition after the last allocation and have been in the coalition through all previous allocations.

DEFINITION 10. $\bar{S} = \{S_t: P_{1k} > = P_1^* \text{ and } P_{2k} < = P_2^* \text{ for } k=1, \ldots, t\}$.

Also, we define a set of finite sequences all of the same length, $\tau$, within $\bar{S}$.

DEFINITION 11. $S^t = \{S_t \in \bar{S}: t=\tau, \tau < \infty\}$.

Defined next are two specific sequences, a complete sequence and an optimal sequence.

DEFINITION 12. A sequence $S_t$ is a complete sequence if and only if $S_t \in S^t$ and $S_{t+1} \notin S^{t+1}$.

DEFINITION 13. A sequence $S_t$ is an optimal sequence if and only if $S_t \in S^t$ and $S^{t+1} = \phi$.

By definition, all optimal sequences must be finite sequences. Therefore, in order to establish that an optimal sequence does in fact exist, it must be proven that finite sequences exist. Theorem 1 establishes that there is a sequence that at some point cannot be extended without destroying the coalition. A proof of Theorem 1 is contained in the appendix.

THEOREM 1. In three-person political coalitions consisting of rational Bayesian actors, dichotomous policy choices, and a leader who seeks to maximize the longevity of his coalition, there exists a finite $t$ such that $S^t \neq \phi$ and $S^{t+1} = \phi$.

The idea behind the proof of Theorem 1 is quite simple once two assumptions are made: each follower is assumed to be able to receive his minimum acceptable payoff from some other coalition with a nonzero probability, and the followers are assumed to have conflicting preferences for 1s. Then, as the number of allocations by *L* becomes large, the variance in each follower's density collapses about $x^*$, so that in the limit at least one follower's subjective estimate of receiving his *MAP* will approach zero and drop below his security level, driving him out of the coalition and ending the sequence.

Theorem 2 (the proof of which also appears in the appendix) states that all complete sequences will be optimal sequences.

THEOREM 2. In three-person political coalitions consisting of rational Bayesian actors, dichotomous policy choices, and a leader who seeks to maximize the longevity of his coalition, if $S_t \in S^t$ and $S_{t+1} \notin S^{t+1}$, then $S^{t+1} = \phi$.

The significance of Theorem 2 is that *any* complete sequence (i.e., a sequence such that both members are still in the coalition after the last allocation, both members have been in the coalition through all previous allocations, and a sequence that cannot be extended without driving one of the members out), will be an optimal sequence. Consequently, in terms of an optimal allocation rule for *L,* the choice of a 1 or a 0 at each step in the sequence is arbitrary as long as the coalition remains intact. The allocation rule, $\Theta^*$, given in Definition 14, requires only that each allocation is made in such a way that, if possible, it does not drive one of the members out of the coalition. Thus, any sequence that proceeds according to $\Theta^*$ will eventually produce a complete and, therefore, optimal sequence.

DEFINITION 14. Let $\Theta^*$ be the rule whereby at each decision point *L* chooses any $z \in \{0,1\}$ such that $S_t \in S^t$.

While allowing for the specification of an optimal allocation rule for the coalition leader, Theorem 2 is also interesting in another respect. Theorem 2 suggests that there may very well be a considerable degree of arbitrariness in the policy process; that is, a leader is predicted to be able to do pretty much as he pleases unless the viability of his coalition is threatened directly. Thus, the model suggests that the political process is such that leaders are held accountable only at certain critical junctures in their leadership, and that the frequency with which these critical junctures occur is a function of the uncertainty and risk characteristics of the members of the coalition.

## The Simulations

In a world where at least one of the coalition members would leave the coalition immediately if the eventual payoff were known with certainty, the important questions are: How long will it take the coalition members to decide that the nature of the world is such that they will not obtain their required payoffs, and why does it take the coalition members as long as it does to come to such a realization? Although our theoretical expectations are that risk attitudes, uncertainty, and polarization all have a bearing on members' decisions to participate in a coalition, we do not know a priori just how important these factors are, or how important they are relative to each other. The purpose of the computer simulations is to provide information about these relationships for a selected set of prior distributions. Our primary hypotheses are that coalitions composed of risk-acceptant members are significantly more durable than coalitions composed of risk-averse members, and that a high level of uncertainty among the members significantly reduces the possibility that a coalition will survive.

### Generation of the Data

All of the data used in the subsequent analyses were generated from a prior distribution with a mean of .6 for each follower; that is, each follower's initial expectation of the leader's true policy position was assumed to be .6, or 60% favorable outcomes.[7] The .6 mean was selected as a reasonable approximation to real-world situations. It is not unrealistic to think each follower initially might expect 60% favorable, albeit diametrically opposed, outcomes. However, as long as we assume that the followers have conflicted preferences, it is increasingly less realistic to believe that each follower will initially expect 70 or 80% favorable, but diametrically opposed, outcomes. Few leaders could be expected to be so skillful, or so ambiguous, as to allow for such divergent beliefs about their true policy preferences. Conversely, one would not expect rational and pragmatic politicians to belong to a coalition in which fewer than 50% favorable outcomes were anticipated.[8]

Given a fixed mean of .6 for the initial prior distributions of the followers, we selected different variances for those prior distributions in order to represent various degrees of uncertainty about the leader's position. The largest variance chosen was based on a prior $n$ of 20, whereas the smallest was based on an $n$ of 100. In total, six different prior distributions were specified, with between approximately 40 and 100 iterations generated for each.[9]

The values of the remaining parameters were fixed as follows. The maximum values for risk and polarization were determined by the points at which only a single additional allocation could be made to each member before the coalition fell apart. The minimum value for the risk attitude was set at .51 under the assumption that most coalition members desire slightly better-than-equal odds of obtaining their minimum long-term required payoffs. In addition, the minimum value for polarization also was set at .51, allowing for just a slight contrast in members' preferences.

Model coalitions were constructed using these initial values for risk and polarization, and the allocation process was allowed to proceed until the coalition ended. Before each allocation, the leader (i.e., the computer algorithm) checked to see if it made a difference whether a 1 or a 0 was allocated. If it made no difference, that is, if an allocation of one type or the other would not drive one of the members out of the coalition, a 1 or a 0 was allocated randomly. Then, the members' prior distributions were adjusted accordingly, and another allocation was made. When the coalition finally ended, the risk and polarization values were incremented one at a time by a value of .01, and the allocation process was restarted for the new coalition.

A separate record of data was constructed for each of the simulated coalitions. Written on each of those records were the values of polarization for each follower (that is, the value of each follower's minimum long-term required payoff), a value for each follower's risk attitude (the required probability of receiving one's minimum long-term required payoff), the initial variance in the prior distribution concerning the leader's posi-

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[7] The computer program used to generate these data was written in PL1 and is available from the authors.

[8] One exception might involve coalitions of small parties, or parties with a very limited agenda.

[9] With a fixed mean of .6, it was found that these priors allowed for a sufficient number of iterations and a sufficient range of iterations. The six initial priors were based on 20, 30, 40, 50, 75, and 100 observations.

**Table 1. Means, Ranges, and Standard Deviations for Independent and Dependent Variables ($N = 566$)**

| Variable | Mean | Minimum | Maximum | Standard Deviation |
|---|---|---|---|---|
| Iterations | 36.9 | 2 | 148 | 34.3 |
| Polarization | .563 | .51 | .59 | .019 |
| Security | .652 | .51 | .94 | .099 |
| Uncertainty | .005 | .0024 | .011 | .0025 |

tion, and the number of iterations or allocations survived by that particular coalition. In order to simplify the data analysis, only symmetric coalitions—coalitions with members having equal but opposite values for risk and polarization—were analyzed.

## Analysis of the Data

The allocation process was stimulated in 566 different coalitions, each consisting of some unique combination of polarization, uncertainty, and security. Table 1 shows various summary statistics for these three variables and the number of iterations each coalition lasted. On average, each coalition lasted roughly 37 iterations; the maximum was 148 and the minimum just two.[10] The security variable exhibited much greater variance than either the uncertainty variable or the polarization variable; and coalitions reaching a polarization of .60 generally were not able to survive one iteration, even when security levels were quite low and uncertainty was low.

In order to evaluate the impact of each of the three variables on iterations, the total number of iterations lasted by each coalition was regressed on polarization ($P$), level of security ($S$), and uncertainty ($U$). The results of this regression analysis, based on all 566 cases, are displayed in Table 2. (The coefficients for polarization and security are based on values of the variables ranging from 0 to 100 rather than 0 to 1.) Together, the three variables explain 78% of the variance in iterations.[11] This good fit, however, is not a surprise, because the regression has been applied to data generated by a computer simulation of a logical model. Rather, the good fit is merely a vindication of the algorithm itself.

Especially important is that each of the three variables affects iterations in the theoretically prescribed manner. Higher levels of polarization, security levels, and uncertainty noticeably shorten the longevity of the coalitions. Moreover, both security level and uncertainty exert large enough effects on iterations to mitigate the effects of polarization. Based on the coefficients in Table 2, a polarized coalition ($P = 56.5$) with risk-acceptant members ($S = 52$) and moderate uncertainty ($U = .005$) will last about 26 iterations—just about the same duration as an unpolarized coalition ($P = 52$) with risk-averse members ($S = 80$) and moderate uncertainty. Thus, holding the uncertainty factor constant and varying the security levels of the members is sufficient to make

**Table 2. Regression Results for Simulation Data**

| Variable | Unstandardized Coefficient | Standardized Coefficient |
|---|---|---|
| Intercept | 1370 | — |
| Polarization | - 19.9 | -1.098 |
| Security | - 3.1 | - .900 |
| Uncertainty | -11689 | - .851 |

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[10] Coalitions that would not last even one iteration or coalitions that lasted for 150 iterations or more were excluded from the data. The upper level of 150 was chosen arbitrarily in order to avoid outlier coalitions, that is, coalitions that lasted far beyond the length of the average coalition. This upper bound eliminated only 13 cases.

[11] Logarithmic transformations of the variables increased the $R^2$ value to about .87, indicating that the relationships are not perfectly linear. Nevertheless, for simplicity of interpretation, we present the variables and their coefficients in their original form.

polarized and unpolarized coalitions endure for about equal lengths of time.

Similarly, a very polarized coalition is predicted to last for close to the average iterations of all coalitions when the members are quite confident about the leader's policy position. For example, a polarized coalition ($P=58$) with risk-acceptant members ($S=52$) and very low uncertainty ($U=.002$) is predicted to last for 31 iterations. From a theoretical standpoint, these results are quite useful for understanding when and why polarized coalitions might endure for relatively long periods of time.

Ideally, we would also like to know the *relative* importance of the three variables for explaining coalition durability. The standardized coefficients in Table 2 yield some information about relative importance, but we urge caution in interpreting their values as indications of importance. The independent variables are correlated among themselves, because polarized coalitions must be fairly risk acceptant if the coalitions are to survive even one iteration. Consequently, determining which variables are most important is somewhat problematic. Nevertheless, the beta coefficients in Table 2 suggest that all three variables are of relatively equal importance, with polarization carrying just slightly more weight.

The fact that uncertainty and risk are virtually as important as polarization suggests that they are indeed useful additions to any model of coalition durability. Not only can substantial gains in explanatory power be expected through an examination of the risk attitudes and uncertainty of coalition members, but some of the anomalies resulting from Axelrod's polarization hypothesis can be accounted for as well. The simulations indicate very clearly that polarized coalitions will endure for relatively long periods of time if the members are either risk acceptant or else quite certain about the leader's policy position. Although these theoretical results certainly do not suggest how one might empirically operationalize the concepts of risk acceptance, risk aversion, and uncertainty, they do at least suggest that these concepts are useful starting points for future research.

## An Empirical Application

One can always resort to idiosyncratic explanations to account for instances of open and durable coalitions in the real world. By introducing risk and uncertainty into explanations of coalition durability, however, seemingly disparate and idiosyncratic explanations can be subsumed under a single, general theory. One historical example—the Bidault II (1949-1950) government in France, nicely illustrates this point.

Even though the second Bidault government was an open coalition because of the exclusion of the Gaullists, it managed to endure for a total of nine months, a relatively long time for any government of the unstable and turbulent Fourth Republic. In comparison, the Bidault I government lasted only six months, and the Schuman government only seven months. And, unlike the Bidault II coalition, each of these other coalitions was connected.

The exclusion of the Gaullists, and hence the durability of the Bidault II coalition, fits well with predictions from the theory of risk and uncertainty. Under the theory of risk and uncertainty, a party will be excluded from a potentially connected coalition if that party's behavior is uncertain and unreliable, or if the party is extremely inflexible in its demands (i.e., risk averse). In the early years of the French Fourth Republic, the Gaullists fit both of these conditions—they were both intransigent and unreliable coalition partners.

Much of the apparent intransigence of the Gaullists resulted from personal characteristics of General de Gaulle himself. He was perceived by many to be arrogant and unyielding, owing in part to his military background. Williams (1964, p. 133), in one description of de Gaulle in 1946, wrote that, "Conciliation and compromise had never been marked features of his character, and no doubt he made too little allowance for the difficulties of partners who had to meet a totalitarian and demagogic rival on the electoral battlefield." Indeed, de Gaulle had a difficult time making the transition from the battlefield to the political arena. Only three months after the 1945 election, an impatient de Gaulle, frustrated by the difficulties of political negotiation, withdrew from politics and returned to private life.

Contributing to the perceived unreliability of the Gaullists were two factors: de Gaulle's lack of support for the first two constitutional drafts and, later, the inability of his party to discipline its members in parliament. De Gaulle's silence on the first constitutional draft put the MRP (Christian Democrats) in the awkward position of being the chief opponent of the draft, a document largely drawn up by the Marxist parties. Later, when the MRP compromised with the leftist parties in an effort finally to put an end to provisional government, de Gaulle publicly denounced their proposals, thereby once again undercutting and embarrassing the MRP. Naturally, the MRP felt betrayed, because de Gaulle had given them no support either in formulating a constitutional draft or in opposing Marxist influence. When de Gaulle re-entered politics in 1947, launching his Rally of the French People (RPF) in an appeal to reform the constitution and combat communist influence, he attracted the support of many MRP voters. Most of these disaffected voters were convinced that the MRP was incapable of dealing effectively with the Communists, a perception created, not coincidentally, by de Gaulle's repeated lack of support for the MRP. Thus, despite its ideological similarities with de Gaulle, the MRP leadership found not only that de Gaulle's support was undependable, but also that de Gaulle was in fact a threat to their party.

At its inception in 1947, the RPF was claimed to be an "inter-group," not a party, so that deputies from other parties could belong to the RPF and still remain members of their old parties. But it was this heterogeneity of the RPF that contributed to its unreliability as a coalition partner. Even though the RPF captured 150 out of 320 senate seats in the 1948 election, most of those elected were interested only in the electoral support afforded by the popular de Gaulle. When the RPF leadership attempted to enforce discipline among these senators, over two-thirds defected from the RPF cause and returned to their original parties.

Clearly, then, by 1949 the Gaullists had established a reputation of intransigence and unreliability. They appeared to be a party that was not only unwilling to negotiate and compromise, but also a party with uncertain political orientations that could not be relied upon for support on the important referenda questions of the day. It is little wonder, therefore, that Georges Bidault, as leader of the MRP, chose to exclude the Gaullists when forming his cabinet in 1949.

On the surface, this story about the Bidault II government is nothing more than that—just a story, a discrete account of historical events. When viewed in the context of a theory of coalition durability based on risk and uncertainty, however, the story becomes a coherent and general explanation of an important political event. Similar stories can be told about other coalitions. The Liberal Party, for example, appears to have been excluded from the Zoli government (1957-1958) in Italy because of its aversion to risk taking. Axelrod (1970, p. 181) has indicated that the Liberals had "unrealistically high aspirations for cabinet positions." And the Rafi apparently were excluded from Levi Eshkol's (1966) government in Israel because of their unreliability and unpredictability. The Rafi had previously defected from Maipai, Israel's main labor party (Aronoff, 1978). Thus, to the extent that the theory of risk and uncertainty allows one to integrate seemingly isolated stories into a larger, general, and coherent picture of coalition durability, the theory is indeed useful, and because the broad logic of the model offers an intuitively plausible account of real world events, the model holds promise for empirical application.

## Conclusions

Only very recently have political scientists recognized the need to develop models of coalitional processes that are both dynamic and probabilistic. Cioffi-Revilla (1984), for example, has formulated a model of Italian cabinets in which longevity is a random variable, so that the duration of cabinets is viewed stochastically rather than deterministically. Browne, Frendreis, and Gleiber (1984, p. 191) have suggested that "the stability of cabinets is appropriately modelled as a problem of individual decision making under uncertainty." We concur wholeheartedly. Our results suggest quite clearly that uncertainty, as well as considerations of risk, are indeed important additions to models of coalition durability.

We believe that the model developed here captures the essence of much of what politics is about. To our minds, politics is a dynamic process filled with uncertainty. From the standpoint of political leadership, uncertainty provides opportunities for manipulation and deception; or as Downs (1957, p. 83) has eloquently pointed out, "Uncertainty gives rise to persuasion." It is the process of persuasion, springing from uncertainty, and its implications for the durability of coalitions that we have sought to capture.

Additional levels of complexity certainly can, and should, be built into the model as developed here. One useful modification might be to assign policy preferences to the leader, rather than have the leader concerned only about remaining in power. This could be accomplished within the present framework simply by requiring that the leader satisfy some minimal policy expectations of his own in addition to those of the other members of the coalition. The policy preferences of the leader would then enter into the model as one more factor affecting longevity.

A second useful extension, one that would also move the model closer to the real world, would be to weight payoffs differently. Two approaches are possible here. One would be to weight recent payoffs more heavily than earlier payoffs under the assumption that politicians, like voters, are concerned with the question, "What have you done for me lately?" A second approach would be to weight payoffs in essentially a random fashion under the assumption that external events impinge upon coalition members in rather unpredictable ways, but nevertheless ways that make certain outcomes more important than others. This latter modification would also move the model in the direction of an "events approach" as proposed by Browne et al. (1984).

Ultimately, the robustness of the simulation results will be demonstrated only if the results hold up under further development and modifications in the assumptions. In the meantime, however, the results suggest strongly that if one is addressing a world of rational maximizers of expected utility, who do practice some Bayes-like probability estimation procedures, there is substantial explanatory power to be gained by investment in the operational assessment of risk and uncertainty in coalition maintenance situations.

## Appendix

THEOREM 1. There exists a finite $t$ such that $S^t \neq \phi$ and $S^{t+1} = \phi$.

*Proof.* We have the following assumptions:

(i) $P_1^* > 0$ and $P_2^* < 1$,

(ii) $\mu_1 > \mu_2$.

Then, by the two decision rules, if $\lim_{t\to\infty} P_{1t}=0$ or $\lim_{t\to\infty} P_{2t}=1$, the coalition will be terminated. Thus, we show that in the limit, $P_{1t}=0$ or $P_{2t}=1$. We consider just two cases: $x^* >= \mu_1$ and $x^* < \mu_1$. (Notationally, we now shorten $f_i(x; r,t)$ to $f(x)$.)

*Case 1.* For $x^* < \mu_1$, let $\epsilon = ½(\mu_1 - x^*)$, so that

$$\int_0^{\mu_1} f(x)dx >= \int_{x^*-\epsilon}^{x^*+\epsilon} f(x)dx.$$

And in the limit,

$$1 >= \lim_{t\to\infty} \int_0^{\mu_1} f(x)dx$$

$$> = \lim_{t\to\infty} \int_{x^*-\epsilon}^{x^*+\epsilon} f(x)dx.$$

The right-most term in the above inequality, however, must be equal to one, because the mean of each follower's subjective distribution converges in probability to $x^*$. Consequently, the value for the middle term in the above inequality must also be one in order to make the inequality true. Since

$$\int_0^{\mu_1} f(x)dx = 1 - P_{1t}, \lim_{t\to\infty} P_{1t} = 0,$$

and the sequence ends, because $P_1^* > 0$ by (i) above.

*Case 2.* For $x^* > = \mu_1$, we know that $x^* > \mu_2$ by (ii) above. Let $\epsilon = ½(x^* - \mu_2)$, so that

$$\int_{\mu_2}^{1} f(x)dx >= \int_{x^*-\epsilon}^{x^*+\epsilon} f(x)dx.$$

In the limit

$$1 >= \lim_{t\to\infty} \int_{\mu_2}^{1} f(x)dx$$

$$>= \lim_{t\to\infty} \int_{x^*-\epsilon}^{x^*+\epsilon} f(x)dx.$$

Therefore, $\lim_{t\to\infty} P_{2t} = 1$, and the sequence ends since $P_2^* < 1$ by (i) above.

THEOREM 2. If $S_t \epsilon S^t$ and $S_{t+1} \notin S^{t+1}$, $S^{t+1} = \phi$.

*Proof.* As before, assume Member 1 prefers 1s, and Member 2 prefers 0s. Let a sequence, $S_t$, of $r$ 1s and $t-r$ 0s be denoted by $S_t = (r, t-r)$.

If $\mu_1 > \mu_2$ (as in Theorem 1), then $S_t \epsilon S^t$ and $S_{t+1} \notin S^{t+1}$ implies

(i) $P_{2t+1} > P_2^*$ if $S_{t+1} = (r+1, t-r)$.

Similarly, $S_t \epsilon S^t$ and $S_{t+1} \notin S^{t+1}$ implies

(ii) $P_{1t+1} < P_1^*$ if $S_{t+1} = (r, t+1-r)$.

First, let $S'_t$ be any sequence such that $S'_t \epsilon S^t$, where $r'$ is the number of 1s in $S'_t$. If $r' > r$, then $r'+1 > r+1$ and $S'_{t+1} \notin S^{t+1}$ by (i) above. And if $r' < r$, then $r'+1 <= r$ and $S'_{t+1} \notin S^{t+1}$ by (ii) above.

Consequently, $S'_{t+1} \epsilon S^{t+1}$ requires $r < r'+1 < r+1$ which is impossible. Hence, if $S_{t+1} \notin S^{t+1}$, $S'_{t+1} \notin S^{t+1}$.

Next, let $S'_t$ be any sequence such that $S'_t \epsilon S^t$ and $r'=r$, but let the order of 1s and 0s in $S_t^1$ differ from the order in $S_t$. Then $P'_{it} = P_{it}$ since by Definitions 6 and 8 $P_{it}$ depends only on the parameters $r$ and $t$, not on the order of the $t$ elements. Hence, if $S_{t+1} \notin S^{t+1}$, then $S'_{t+1} \notin S^{t+1}$ and $S^{t+1} = \phi$.

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# The Decline of Class Revisited: Class and Party in England, 1964-1979

JONATHAN KELLEY
IAN MCALLISTER
ANTHONY MUGHAN
*The Australian National University*

*Class has long been the preeminent source of political conflict in industrial society, but its electoral influence has declined in recent years. The sources of the decline are not yet firmly established, and moreover the implications for political parties remain unclear. The decline-of-class hypothesis states that parties on the left will decline as the working class becomes more affluent and adopts middle-class styles of conduct. By contrast, the party-appeals hypothesis suggests that as the electorate becomes more middle class, parties of the left will alter their appeals to encompass the growing middle class and so offset the shrinkage of their traditional working-class constituency. This article applies multivariate anaysis to survey data collected in England between 1964 and 1979 to test four specific hypotheses derived from the two scenarios. The results support the decline-of-class theory's prediction that economic development erodes the working-class bases of left-wing parties, but not its claim that the left-wing party's vote declines proportionately. Rather, the results suggest that parties are apparently able to change their appeals to reduce their losses, as argued by the party-appeals theory, but not to eliminate them. It seems that there are restraints on parties' ability to change their appeals, limitations not envisioned by the party appeals theory.*

Historically, social class emerged as the primary basis of political conflict in many western societies after the Industrial Revolution. With the growth of a large, urban working class in the late nineteenth century, parties of the left emerged to make explicit appeals based on working-class interests (LaPalombara & Weiner, 1966; Lipset & Rokkan, 1967). In many countries these appeals were eventually successful, sweeping the left into power for long periods. But at the apogee of these parties' power in the decades after the Second World War, sustained economic development began to erode their social base. With improving material standards of living, growing white collar employment, and the pervasive diffusion of middle-class styles of behavior, it seemed that many "natural" working-class supporters of the left were becoming middle-class and eventually would defect to parties of the right. This decline-of-class theory gained wide currency in the 1960s and was used to underpin many works speculating on the transformation of party systems and the nature of politics in general in advanced industrial democracies (see, for example, Abramson, 1971; Alford, 1963; Bell, 1960; Goldthorpe, 1969; Korpi, 1978; Lipset, 1964; Lockwood, 1960; Myers, 1970; and Runciman, 1964).

Underlying all of these works were two fundamental theses:[1]

HYPOTHESIS 1. Economic growth reduces the

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[1] An alternative argument is that class cleavages in politics have declined because objective differences between classes decline with economic development. Although this is not an implausible argument, in fact class cleavages seem to have been stable, at least since the Second World War. The best evidence on this question is from studies of the life-chances of children born into different classes, that is, studies of social mobility, since there are baseline data from the early postwar period and, in addition, the topic lends itself to retrospective inquiries. Evidence from the United States, Britain, Canada, and industrial societies generally all suggest that there has been little or no consistent change (Blau & Duncan, 1967, pp. 177-182; Goldthorpe et al., 1980; Hauser et al., 1975; Hazelrigg & Garnier, 1976; Hope, 1981; McRoberts & Selbee, 1981).

size of social groups that traditionally support left-wing parties.

According to this hypothesis, the vote for the left should decline proportionately because no modification of the "normal," long-established link between class and party is anticipated by it:

> HYPOTHESIS 2. The electoral strength of left-wing parties declines in direct proportion to the shrinkage of the social groups that traditionally support them.

There is, however, a contrary view based on the argument that the appeals political parties make, and hence their sources of electoral support, are not fixed and immutable but rather are the result of conscious, rational choices by party elites. In a classic analysis, Downs (1957, p. 28) argued that parties rationally choose to maximize their vote by appealing to the greatest number of electors: "Parties formulate policies in order to win elections, rather than win elections in order to formulate policies." Rather than being mere passive recipients of support from existing social groups, parties are active agents, consciously choosing what political stance they will adopt (Sartori, 1969, 1976). This argument implies that parties of the left will not stand passively by as their traditional supporters become less numerous. Instead they will try to make good the loss by increasing their support in the growing middle class, either through an implicit appeal to middle-class interests or, with less risk of alienating traditional supporters, by eschewing class altogether and emphasizing issues (such as the environment, nuclear energy, or women), which cut across class. As a result, the political distance between the middle and working classes will narrow as parties of the left shift their ground.

The logic of Downs's argument goes further, implying that, in spite of the changes imposed by economic growth, the vote for the left will not in fact decline. Drawing on economic theories about the optimal geographical location of competing firms, Downs shows that the equilibrium position for two parties contesting the vote along a single left-right dimension leads to an approximately equal division of the vote, with one party offering an appeal somewhat to the left of the center and the other offering one somewhat to the right. If the population shifts to the right, the left party must, for fear of losing votes, shift its appeal to the right, and, crucially, the conservative party must also shift its appeal to the right for fear of losing more from its normal right-of-center constituency (now closer to the left's new position) than it could gain by not moving. Hence, the left is able to make new converts on its side of the new center and the equal division of the vote is reestablished. In short, this party-appeals theory, as we shall call it, argues that:

> HYPOTHESIS 3. Economic growth leads parties of the left to mute their appeal to the working class and hence reduces class polarization in politics.

and

> HYPOTHESIS 4. The division of the vote between left- and right-wing parties is unaffected by economic growth.

The decline-of-class theory and the party-appeals theory thus lead to several important and empirically testable propositions about the effects of economic growth on politics. Two of these propositions are different but perfectly compatible (Hypotheses 1 and 3), whereas the other two are contradictory (Hypotheses 2 and 4). In this article we test these four hypotheses by analyzing the changing relationship between class and party in England over the period 1964 to 1979.

## The English Case

England is an important test case for these theories because the British system is the prototypical example of class-based politics, as well as one of the oldest democratic systems and the source of many of the political traditions and institutions of the Anglo-American world. The dominant class alignment emerged between the First and Second World Wars (Wald, 1983); it is best documented in a series of analyses published in the 1960s, all of which contradicted the decline-of-class theory and validated the frequently quoted aphorism: "Class is the basis of British party politics; All else is embellishment and detail" (Pulzer, 1967, p. 98; see also Abrams & Rose, 1960; Alford, 1963). So strong was the electoral influence of class that whatever minor shifts of support took place, its impact seemed relatively consistent from election to election. Despite the alternation of parties in government, class remained by far the most significant determinant of voting choice in the British electorate, thereby underpinning the general stability of the party system.

Over the past decade, however, this static view of the relationship between class and party, and its political consequences, has changed profoundly as the relationship itself has weakened. Butler and Stokes (1974, p. 203), for example, see "the declining strength of the association between class and party (as) one of the most important aspects of political change in this decade." Others have reached similar conclusions (Alt, 1984; Cain, 1978; Crewe, Sarlvik & Alt, 1977, p. 167; Franklin, 1982; Franklin & Mughan, 1978; Miller, 1978, pp. 263-264; Rogowski, 1980; Rose, 1980, p. 149). Even though there may now be widespread support for the first part of the decline-of-class thesis, the second, that is, that Labour must inevitably lose support, remains less clear. Several studies of attitudinal changes in the mass public are consistent with the view that the Labour party's support must decline (Crewe, Sarlvik, & Alt, 1977; Goldthorpe et al., 1971; McKenzie & Silver, 1968; Nordlinger, 1967), but it has also been argued that both parties have suffered equally in the face of economic and social change (Crewe, 1977). It is apparent, therefore, that there has been no rigorous empirical test of the overall thesis, much less any plausible estimate of the magnitude of the effects of this change. One aim of this study is to fill both these gaps.

Another major question concerns the specification of which aspects of class are changing, and with what impact upon voting choice. It is now generally agreed that several dimensions of class are politically salient in Britain, notably occupation, union membership, education, class self-image, and home ownership.[2] Previous analyses, such as Butler and Stokes's (1974) classic, *Political Change in Britain,* typically look only at one class variable (most commonly class self-image or occupation dichotomized into manual versus nonmanual), and trace changes in its relation to the vote over time. This approach is inadequate in that it neglects other important aspects of class and, insofar as there are changes in other correlated aspects of class, estimates based on such a simplistic model will necessarily be biased. For example, an increase in education or middle-class self-image among people who are objectively manual workers will make it seem that manual workers are no longer so politically different from nonmanual workers, even if in fact there is no such change (because the manual group is increasingly "contaminated" by well-educated workers who identify with the middle class). Our analysis circumvents this problem by using powerful multivariate techniques to deal simultaneously with the several politically relevant aspects of class.

Previous estimates have also been flawed by a failure to deal with several other politically crucial variables which are deeply entwined both with class and with recent historical changes. First, and particularly central to the decline-of-class thesis, is the very important dampening effect on change produced by political socialization. As parties of the left came to power after the Second World War, an increasing proportion of children, being raised in left-wing homes, became strongly disposed to support the left (Butler & Stokes, 1974, pp. 229-230; Jennings & Niemi, 1978; Rose, 1974). This change continues unabated, so that new entrants to the British electorate come increasingly from established Labour families. We take this potentially confounding change explicitly into account in our analysis and provide precise estimates of its effects. Second, there are other important changes in society that may affect the relationship between class and party. In particular, trade union membership mediates this relationship and, importantly, it has been spreading from the manual workforce into white-collar occupation (Bain, 1970). This growth in union membership should redound to the overall benefit of the left, so it too should be controlled in the analysis. Again, we provide estimates of the effect of this change on the Labour vote.

After describing the variables in our model of class voting and our methods of analysis, we first present the model estimated over the pooled 1964, 1970, 1974, and 1979 surveys in order to provide a general picture of the enduring character of the class-party relationship in England during this 15-year period. We then present an analysis of the changes over the four elections and, finally, discuss the implications the findings have for the decline-of-class and party-appeals theories.

## Data, Measurement and Method

### Data

The analysis is restricted to England because popular support for Scottish and Welsh nationalism in 1974 and 1979 means that the party systems in these countries are no longer strictly comparable: "The growth of nationalist voting (in Scotland and Wales) and national trends in support for non-nationalist parties imply that a Britain-wide analysis would simply mix and confuse a variety of divergent trends" (Miller, 1973, p. 257; see also Drucker, 1979). The time span covered is 1964 to 1979, the maximum time period for which suitable data are currently available. For the sake of brevity, we analyze change over only four of the six general elections that have taken place over this period: 1964, 1970, October 1974, and 1979. Although this is a relatively short period in which to study the changes resulting from economic growth, it is nonetheless long enough for real changes in standard indicators of it to manifest themselves, and our estimates of the effects of these changes provide the best evidence yet available concerning the likely impact over a longer period of change (see Table 1).

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[2] In this article, we unfortunately have not been able to consider class in either Marx's sense of ownership of the means of production or Dahrendorf's sense of authority (e.g., Robinson & Kelley, 1979). The required questions were not asked in all of the four surveys we analyze so, to maintain comparability over time, we were forced to omit them throughout, although we have dealt with them at some length elsewhere using only the 1979 data (Kelley & McAllister, in press). The present results are therefore restricted to class in a limited sense of the word, albeit the sense in which it is traditionally used in British political sociology.

The data come from four separate surveys: the 1964 and 1970 electorate cross-sections of Butler and Stokes (1974) (*N*s = 1,769, and 1,845); the October 1974 British Election Study (*N* = 2,365); and the 1979 British Election Study (*N* = 1,893). Data for the 1983 election are not available at the time of writing. Each survey involved a randomly drawn sample of the British electorate and was put into the field immediately after the general elections of those years: For comparability, we have used the 1964 post-election study in preference to the 1963 pre-election study. Completion rates for the surveys were 68%, 70%, 75%, and 72%.

The analysis was restricted to those who voted (approximately three-quarters of the total) and reported their class identification and their (or their spouse's) occupation. With these restrictions, the *N*s are 1,208, 1,186, 1,503, and 1,207.

## Measurement

The dependent variable is the recalled vote in the general election that had just been held. It is coded "1" if voted Labour and "0" if voted Conservative. Liberal voters were coded ".5," thereby in effect placing them halfway between Labour and Conservative, which is reasonable in terms of their attitudes and social characteristics (Alt, Crewe, & Sarlvik, 1977; Himmelweit et al., 1981, chap. 10; Lemieux, 1977). Alternative coding schemes (e.g., combining them with Conservatives or omitting them from the analysis altogether) led to the same substantive conclu-

**Table 1. Variables, Scoring, and Means**

| Variable | Scoring | Means 1964 | Means 1970 | Means Oct. 1974 | Means 1979 |
|---|---|---|---|---|---|
| Vote | | | | | |
| Labour vote | 1 = Labour; .5 = Liberal;<br>0 = Conservative | .54 | .49 | .52 | .43 |
| Family background | | | | | |
| Father white collar worker | 1 = white collar worker<br>0 = otherwise | .20<br>(.21) | .23<br>(.21) | .25<br>(.25) | .28<br>(.28) |
| Father Labour partisan | 1 = Labour; .5 = Liberal<br>0 = Conservative | .51 | .56 | .56 | .56 |
| Class position | | | | | |
| Head of household white collar worker | 1 = white collar worker<br>0 = otherwise | .37<br>(.34) | .39<br>(.34) | .40<br>(.40) | .42<br>(.41) |
| Owner-occupier house tenure | 1 = owner-occupier<br>0 = otherwise | .39 | .52 | .57 | .61 |
| Class self-image[a] | 1 = middle class self-image;<br>0 = working class self-image | .32<br>(.30) | .33<br>(.36) | .34<br>(.34) | .35<br>(.34) |
| Education[a] | Years of full-time schooling | 9.81<br>(9.64) | 10.03<br>(10.41) | 10.18<br>(10.00) | 10.36<br>(10.32) |
| Other[b] | | | | | |
| Trade union member | 1 = member<br>0 = otherwise | .22 | .26 | .25 | .31 |

[a]The means for these variables are estimates, for the reasons noted in the text. See note 3 for details of their calculation. The actual means are shown in parentheses.

[b]For simplicity, age, sex, and region are omitted. Their effects are uniformly small and generally statistically insignificant. Including them has no noticeable effect on the results for the other variables.

*Sources.* Sources for all tables are: 1964 and 1970 electorate cross-sections of Political Change in Britain Survey (Ns = 1,769 and 1,845); October 1974 and 1979 British Election Studies (Ns = 2,365 and 1,893).

sions (details available on request). Nonvoters and those who did not answer the voting questions, a little over 20% of the total, are omitted from the analysis.

The variables used in the analysis, together with their means, are given in Table 1. The independent variables were selected because of their proven or potential impact on party choice and can be divided into three categories. The first, family background, is measured by father's occupation and partisanship. Mother's partisanship was not available in all the surveys and was therefore excluded. The second, the respondent's class position, is measured by four variables. Occupation is measured by the categorical distinction of whether or not the head of the household is a white-collar worker; a continuous measure of occupational status has no effect on voting (Kelley & McAllister, in press). The other dimensions of social class are tapped by type of housing tenure, class self-image, and education. Type of housing tenure is included because the division between owner occupiers and council tenants has proved to be an important and enduring electoral influence in Britain. Butler and Stokes (1974, p. 107), for example, suggest that "housing has more to do with defining the sub-cultures of social class than all else but occupation itself." Income is excluded because it has no appreciable effect on the vote once other variables are considered. The final variable is trade union membership, which is included for the reasons noted above. For simplicity, age, sex, and region, which turn out to be of little or no political relevance, are excluded from the analysis. The correlations and standard deviations for these variables are given in Table A1.

The analysis is based on ordinary least squares regression methods, which assume that relations are, to a reasonable approximation, linear and additive (Hanushek & Jackson, 1977; Kmenta, 1971). There are well-known reservations about using regression when one dependent variable is dichotomous, but these difficulties are in practice minimal unless the marginals are highly skewed, which is not so in this case even when Liberal votes are omitted to make it dichotomous (Gillespie, 1977; Kmenta, 1971, pp. 425-427). Missing data (save on vote, occupation, and class self-image) are treated by the "pairwise present" procedure, which is statistically preferable to the usual alternatives (Hertel, 1976). Alternative analyses using listwise deletion lead to identical substantive conclusions (details available on request). For the analysis of Figure 1, a single model is used only for heuristic purposes because, as we show, there are differences in slopes in different time periods. The central results (given in Figures 2 through 6 and Table A2), are based on models estimated separately for each survey, in effect allowing for all possible interactions between date and other variables.

Multicollinearity is not a serious problem here, because the highest correlations between independent variables are approximately .4, which is well below the very conservative figure of .5 suggested by Hanushek and Jackson (1977, p. 89). Nor do minor changes in model specification or measurement lead to any appreciable changes in the estimates, another clue to the absence of serious multicollinearity (Hanushek & Jackson, 1977, p. 90).

Although England has undoubtedly become steadily more middle class, there remains some uncertainty in estimating the magnitude of these small changes from survey data because of sampling error, differences in sample coverage, and differences of detail in the way the questions were asked and coded. We therefore assume that the actual data for the four surveys represent imperfect estimates of the underlying trend and attribute the minor fluctuations observed in them to problems inherent in the data. Specifically, we argue that this will affect four variables—father's occupation, head-of-household's occupation, class self-image, and education—and that changes in each will reflect long-term shifts in the economy, whereas any short-term fluctuations or reversals of the trend are unlikely. We therefore estimate the underlying trend by fitting an ordinary-least-squares regression line through the observed means, and use these predicted means in all substantive analyses.[3] By contrast, small fluctuations in trade-union membership and house tenure are more likely to be genuine, because they can be influenced by short-term political and socioeconomic change, so we have not smoothed trends in these variables. The use of actual instead of fitted means leads to some differences in our results but leaves the major conclusions unchanged (details available on request).

[3] The predicted means were calculated as follows. For example, the observed mean years of schooling are 9.64 in 1964, 10.41 in 1970, 10.00 in October 1974, and 10.32 in 1979. We treated these as four data points and estimated a linear regression predicting education on the basis of date obtaining:

$$\text{Estimated Schooling} = -61.63 + .03638\ (\text{Date of Survey}).$$

This implies an annual growth in the average level of schooling in the British population of slightly under 4% of a year, or something over a third of a year per decade. Evaluating the regression equation for various dates yields predicted mean years of schooling of 9.81 years in 1964, 10.03 in 1970, 10.17 in 1974 and 10.36 in 1979.

## Class Voting in England

### The Model

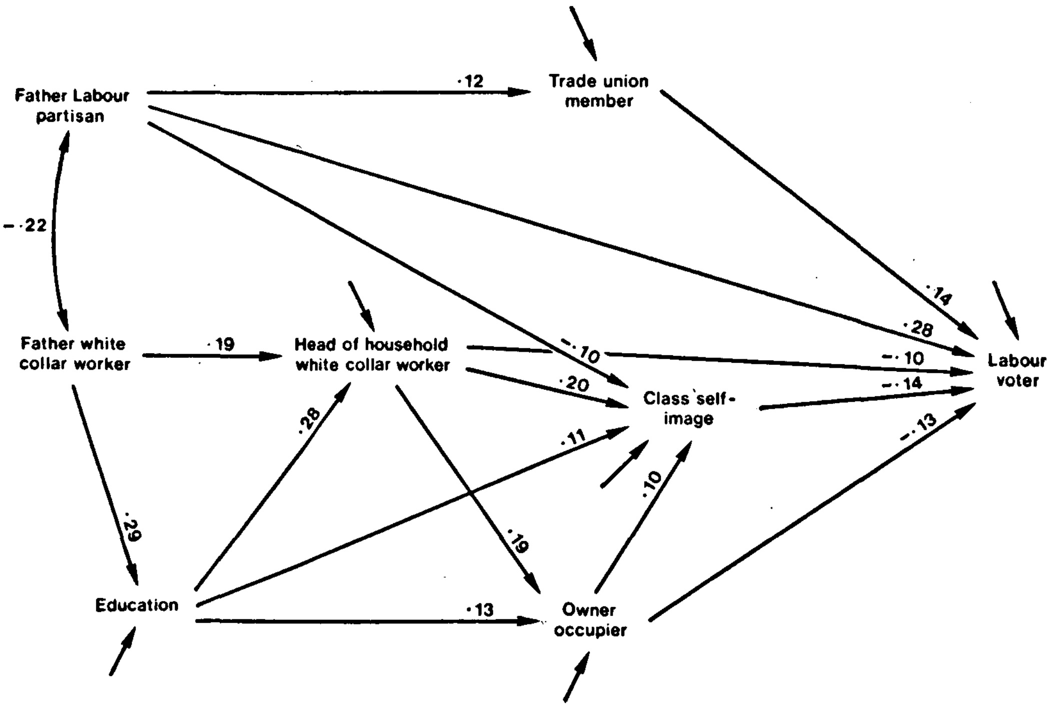
Before the question of change can be addressed, we need to specify how the various dimensions of social class stand in relation to both each other and to the dependent variable, party choice. It is also useful to identify which of them have a significant impact on party choice in the period from 1964 to 1979. Our model (Figure 1) is fitted to the 1964, 1970, October 1974, and 1979 surveys simultaneously and so (for the moment) ignores changes over time. The model shows the equivalent of standardized partial regression coefficients (betas, or paths) that are directly comparable so that the relative importance of the various dimensions of class can be assessed. For clarity, only effects of the magnitude of .10 or more are shown here (although all are included in subsequent analyses). Figure 1 adds little to the conventional wisdom about electoral behavior in general and British electoral behavior in particular. The single most important determinant of voting choice is respondents' political socialization, as measured by father's partisanship, whereas father's occupation influences the vote only indirectly through its impact on respondents' education and occupation. Class self-image, home ownership, and occupational class are of roughly equal direct influence. Education has only an indirect influence on the dependent variable. This model is the basis for our analysis of the changes in the class-party relationship between 1964 and 1979.

### The Weakening Relationship between Class and Party

We can see the extent to which social structure has changed in England between 1964 and 1979 by comparing the means of the class variables for the four surveys.[4] Figure 2 shows there has been a

**Figure 1. A Basic Model of British Electoral Behavior, with Standardized Partial Regression Coefficients for 1964, 1970, October 1974 and 1979 Pooled Sample.**

Only major influences are shown ($\beta \geqslant .10$).
All coefficients shown are significant at $p < .01$; N = 15,104.



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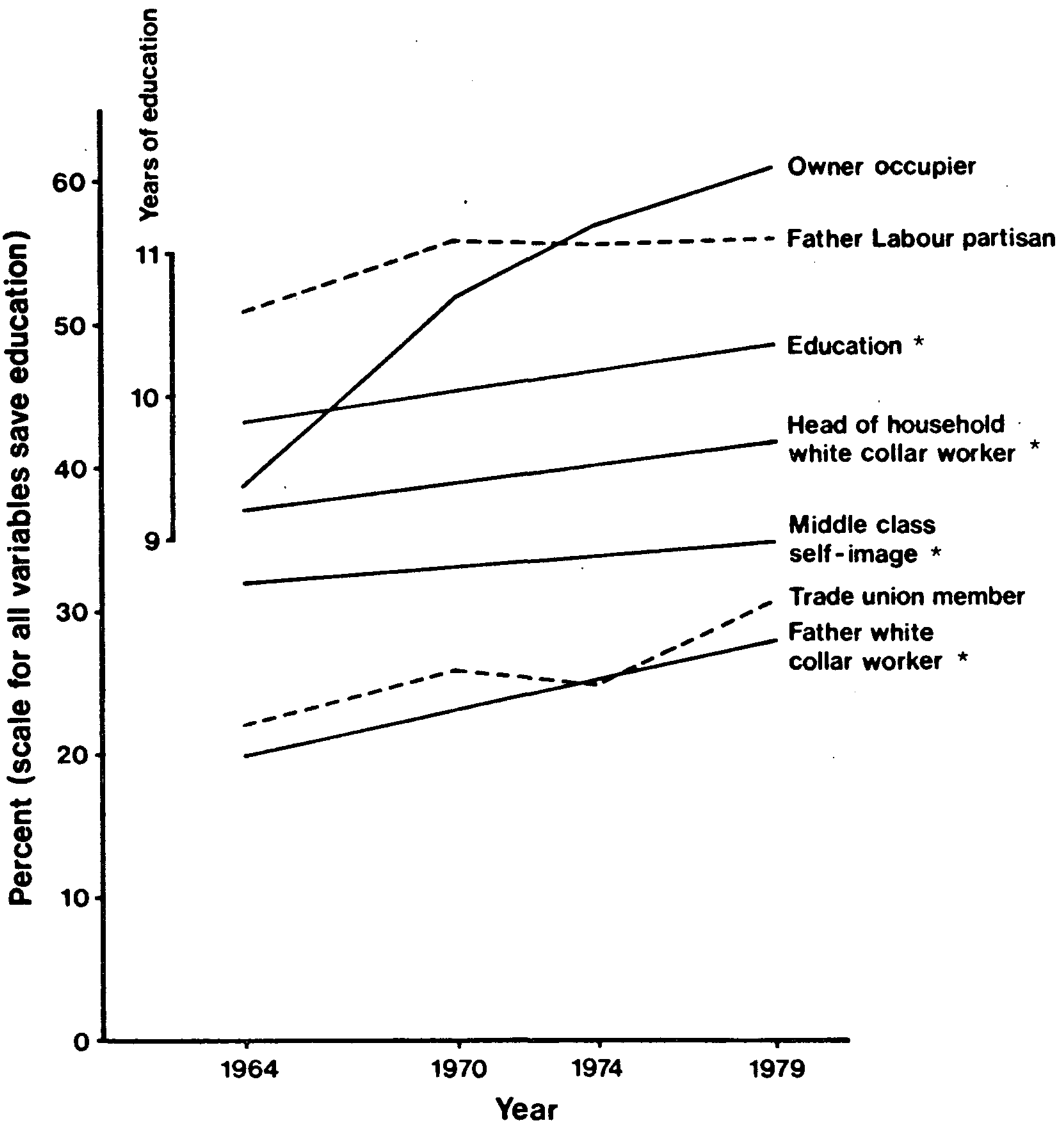
[4] We take 1964 as the baseline, which seems a reasonable choice conceptually—there was apparently no substantial change in the relation between class and party in the decades preceding it (Alford, 1963)—and a necessary choice practically, because there were no suitable national surveys in the 1950s or earlier.

slow but steady embourgeoisement of the English electorate (see also Halsey, 1972; Rose & McAllister, 1982). For example, in 1964, 37% of the electorate were in white-collar occupations; by 1979 this number had increased to 42%. Similarly, in 1964 the average voter had 9.8 years of full-time education, compared to 10.3 years for his 1979 counterpart. The largest increase has occurred in home ownership: in 1964 just under 40% of voters were owner occupiers, compared to 60% in 1979.

In addition to these changes in class variables, there is a small but politically crucial increase in the proportion of the electorate coming from Labour homes (up 5%). Trade union membership appears to be marginally more susceptible to government policy, since it declined slightly during the 1970 to 1974 Heath Conservative government and resumed its slow upward movement when Labour regained power.

The decline-of-class theory was correct, then, in holding that the working class slowly shrank in

**Figure 2. Changes in the Mean for Various Socioeconomic Variables (Solid Lines) and Other Variables (Broken Lines) in England, 1964-1979**



size between 1964 and 1979. But what were the political implications, if any, of this shrinkage? Let us begin by asking (hypothetically) what would have happened if the 1964 relation between class and the vote had subsequently remained unchanged in strength. To take education as an example, in 1964 each year of education reduced an elector's probability of voting Labour by about 7%. As we have just seen, educational levels were increasing and by 1970 the average voter had an additional half a year of schooling. So if the 7% educational differential remained unchanged, then the Labour vote should, hypothetically, have dropped by 3.5% (7 times the one half, shown by the solid line in Figure 3). That is a very substantial drop in electoral terms, representing more than the winning margin in two-thirds of the general elections in this period. Other things being equal, it alone would make it very difficult for Labour to gain power again and would dramatically support the decline-of-class theory's Hypothesis 2.

Labour did regain power, however, and it did so in part because the relationship between education and voting changed dramatically, the 7% differential in 1964 falling to approximately 2% in subsequent elections (see Figure 4). So the loss to Labour was much less than it would have been, amounting to about 1% in 1979 (namely, the differential of 2% times the half-year increase in education, with the results shown by the broken line in Figure 3). This is as much as the class-appeals hypothesis would lead us to expect, and the most likely explanation is that it is the pay-off of the Labour Party's conscious decision to tone down its earlier appeal to less-educated voters (Kavanagh, 1982, pp. 104-107). Whether Labour managed to defuse the issue entirely is, however, another question. The estimated effects for 1970, 1974, and 1979 (shown in Figure 4) are approximately 2%, which implies a slight loss to Labour. Although small, all three are significantly different from zero at $p < .05$. In other words, the evidence suggests that the growth in educational levels still worked somewhat to Labour's disadvantage despite the party's attempts to neutralize it.

Changes in other aspects of class, with the exception of home ownership, had much less of an impact than changes in education; none would have led to a change of as much as 1% in the Labour vote had 1964 patterns prevailed. This proviso notwithstanding, however, the pattern is similar to that for education. Both the growth in the white-collar workforce and the increase in the proportion of the population identifying with the middle class would have harmed Labour had (hypothetically) the link between class and vote remained unchanged after 1964. Changes in home ownership, by contrast, were more harmful in their impact than changes in education. Figure 2 shows that the increase in home ownership was dramatic, and the difference in voting behavior between owner occupiers and council house tenants increasingly large (Figure 4). But, unlike on other class questions, the Labour Party seems not to have been able so easily to mute its class-based appeal, so that although differences based on home ownership declined in 1970, they rose dramatically toward the end of the 1970s. Indeed, by 1979, Labour lost more votes among home owners than it would have done had 1964 patterns persisted (see Figure 3).

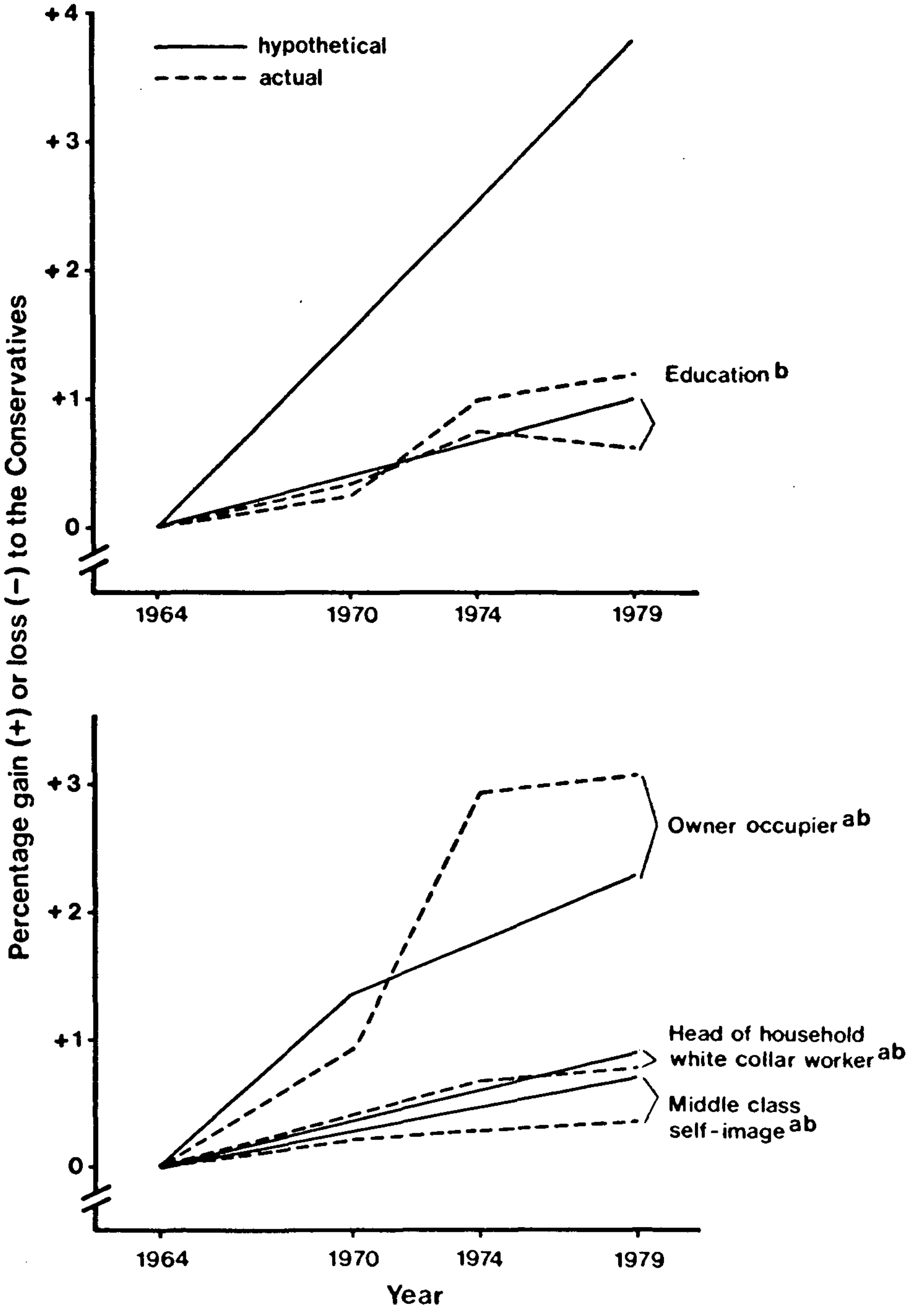
This is clear evidence against the basic premise of the party appeals theory set out in Hypothesis 1. But it is an informative exception to the general pattern. On most class issues it seems that the Labour Party makes the running, which is perhaps not surprising because the party originated from a working-class base and has consistently exploited class issues. The Conservatives, on the other hand, have for generations studiously ignored such issues and raised other themes, such as leadership, patriotism, and religion because they would lose any election fought on strictly class lines. Consequently, when the Labour Party mutes its previously strident appeals to class interests, the Conservatives cannot easily retaliate by stressing class concerns themselves. The Labour Party, in contrast, is able to change its emphasis relatively quickly and thereby reduce the impact of unfavorable changes in the class structure.

But housing is different from the traditional aspects of class. In Britain there are two dominant types of housing: owner-occupied and rented council housing, the latter heavily subsidized by the state. Owners are traditionally Conservative in partisanship, and council house renters traditionally Labour (Butler & Stokes, 1974, p. 107; Rose, 1982, Table 2). However, home ownership is widely valued in the working as well as in the middle class, and the Conservatives have exploited the issue's cross-class appeal to dominate the housing question. First, they have strongly encouraged home ownership and were the main architects of the dramatic growth in home ownership in the 1960s and the early 1970s. Second, in the early 1970s they introduced the policy of selling council houses to their tenants at reduced prices, often over the vocal opposition of Labour-controlled local councils (Berry, 1974). As a result, the Conservatives have increased the electoral salience of housing to their own advantage (Dunleavy, 1979; McAllister, 1984).

By contrast, Labour has traditionally supported council housing, originally as a social welfare measure, and has long been identified with opposition to Conservative initiatives to reduce the size

**Figure 3. Conservative Gains Due to Socioeconomic Changes in England, 1964-1979.**

Solid lines are hypothetical gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.



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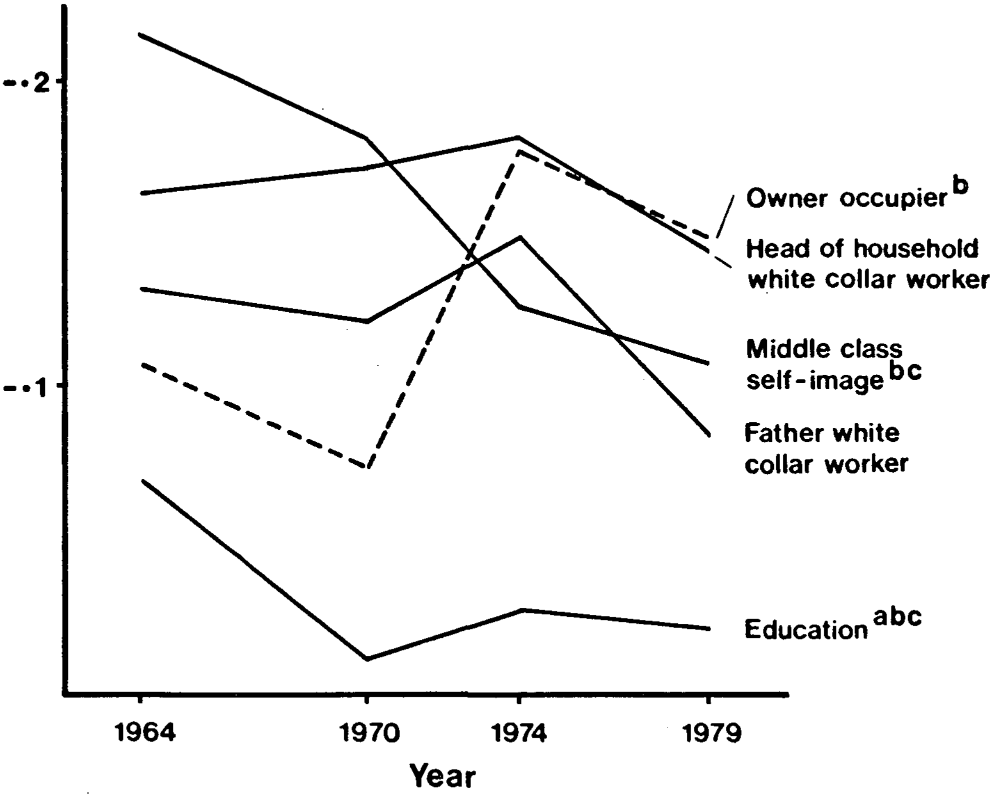
[a,b] 1979 hypothetical value (a) or actual value (b) significantly different from 1964 at $p < .05$, one tailed.

of the council rental market. So Labour cannot reverse its stance on housing without alienating many of its supporters. The Conservatives therefore hold the initiative on the housing issue, while Labour has few options, the reverse of the situation on the other class issues. Further, for good vote-maximizing reasons the Conservatives have stressed their appeal to home owners by advocating lower interest rates in home loans and introducing a policy of allowing council tenants to purchase their homes at advantageous prices. The overall result has been that the Labour Party lost even more home-owner votes than would have been predicted by structural change alone.

The same general picture emerges when we examine the electoral impact of the two changes that might reasonably have been expected to increase the Labour vote; these are the increase in the proportion of trade union members in the electorate and the increase in the proportion coming from a Labour household. Figure 5 shows that the Labour Party in fact benefited by approximately 1% of the vote in 1979 from the increasing unionization of white-collar workers, who have tended to be more militant than their blue-collar counterparts (Bain, 1970). The increasing number of people born into Labour households can also be seen to have benefited Labour by approximately 2% of the vote (broken line), but there has been a slight decrease in the influence of family political socialization, so that the party's actual gain is less than would have been expected had the link between father's party and voting choice remained at the 1964 level (solid line).

It is obvious that, except in the case of home ownership, actual Labour losses have been substantially less than they might have been, which leads us to reject the second hypothesis to the effect that economic growth leads to a proportionate decline in the vote share of left-wing par-

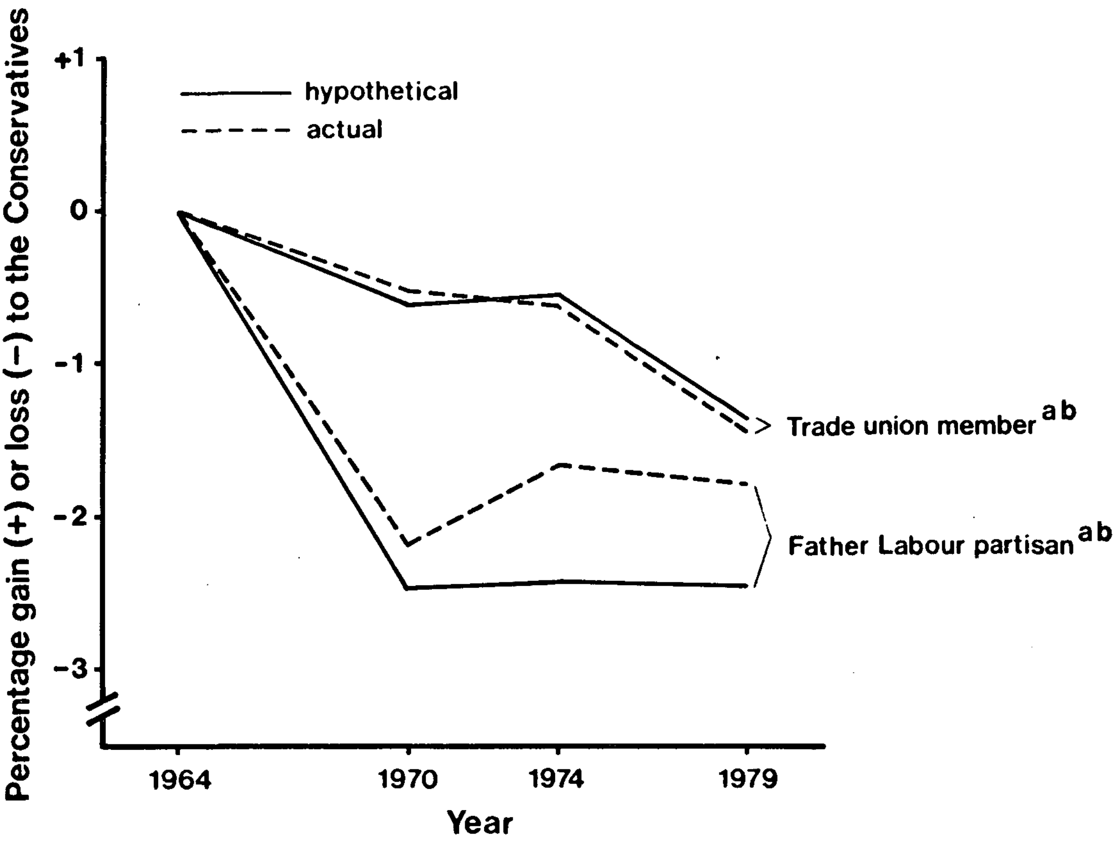
**Figure 4. Metric Partial Regression Coefficients Giving the Total Effect on the Vote of Various Socioeconomic Varibles for England, 1964-1979**



Source: Appendix Table A2, lines 5 to 8.

$^{a,b,c}$1964 coefficient significantly different from 1970 (a), 1974 (b), or 1979 (c) at $p < .05$ (two-tailed).

**Figure 5. Conservative Gains (+) or Losses (−) Due to Various Structural Changes in England, 1964-1979**



Solid lines are hypothesized gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.

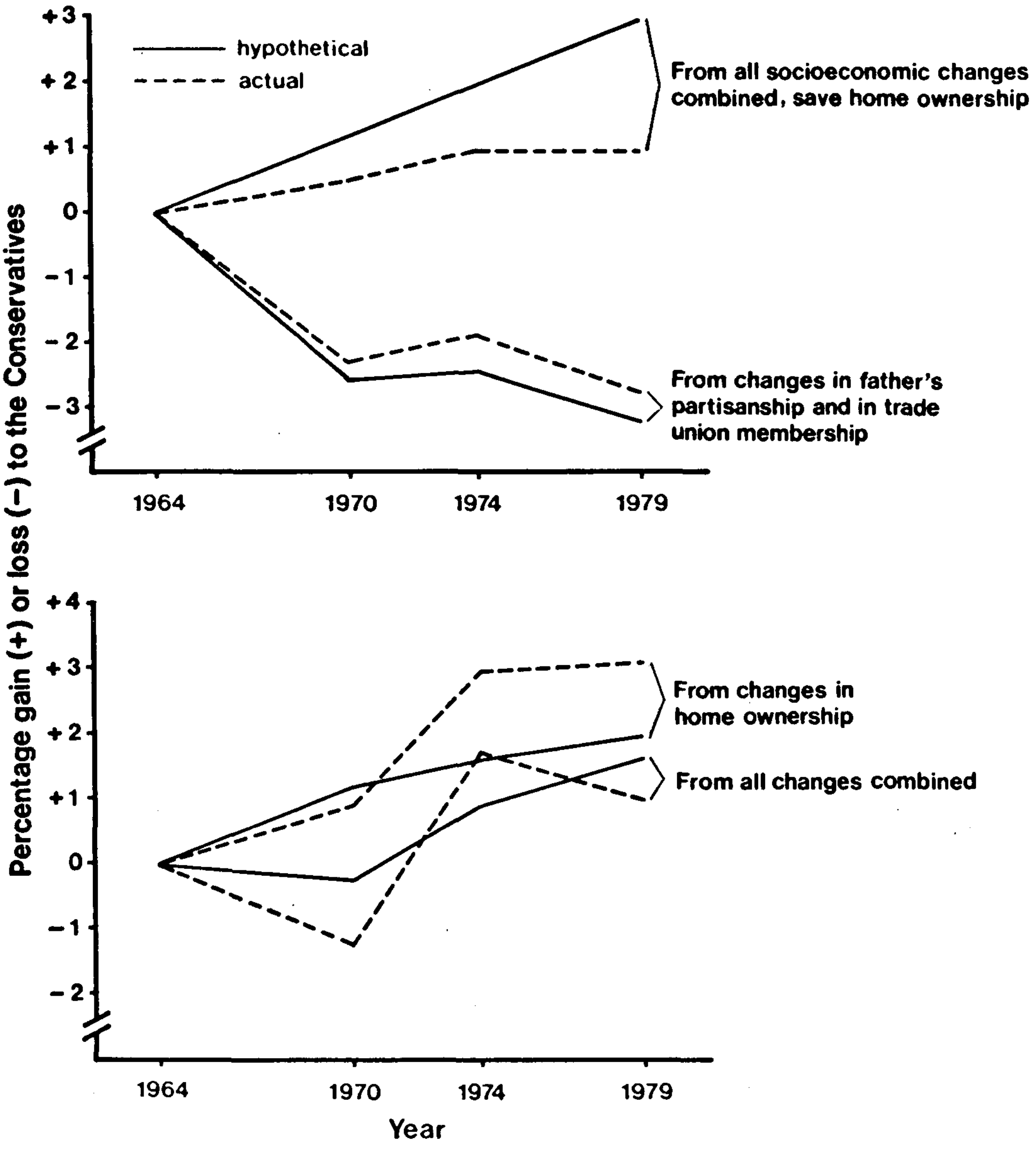
[a,b]1979 hypothetical value (a) or actual value (b) significantly different from 1964 at $p < .05$, two-tailed.

ties. We can gain a further perspective on the influence of socioeconomic change by showing how it affects party support in the aggregate. Figure 6 presents two separate sets of results: in the upper graph the consequences of socioeconomic change (save home ownership) and of changes in father's partisanship and trade union membership, and in the lower graph the consequences of change in home ownership and of all changes combined. From all socioeconomic changes combined (save home ownership), Labour could have expected to lose approximately 3% of the vote if the relationship between class and party had remained at the 1964 level (solid line). But in the event, this relationship did change, and Labour had a smaller net loss of 1% by 1979. Labour did, however, reap an actual net gain of approximately 2.5% in 1979 as a result of father's partisanship and increasing trade unionism. From the bottom graph in Figure 6 we can see that the net result of all of these changes combined was an actual gain to the Conservatives of 1%.

Clearly the electoral decline of Labour is not the inevitable concomitant of economic growth, as the decline-of-class theory (and Hypothesis 2) predicts. Moreover, it is possible to go further and argue that as social change continues to erode their traditional social bases, parties of the left will seek new support within the middle class. These results probably help to explain Labour's success in the years between 1964 and 1979 in gaining substantially more middle-class support than previously (Kavanagh, 1982, pp. 107-108).

**Figure 6. Conservative Gains (+) or Losses (-) from All Sources of Change Combined in England, 1964-1979**

Solid lines are hypothetical gains which would have occurred if the relationship between structure and the vote had remained as it was in 1964; broken lines show actual changes. See appendix for methods.



Indeed, it appears that Labour was able to make most of its inroads into the skilled or supervisory nonmanual grades, rather than the management or professional grades (Rallings, 1975). Our evidence therefore endorses Hypotheses 3 and the party appeals theory, which postulates that economic growth will lead parties of the left to mute their appeal to the working class and strengthen their appeal to the middle class, hence depolarizing the class cleavage.

The final Hypothesis 4 states that the division of the vote between left- and right-wing parties will be unaffected by economic development. This is, in effect, a logical extension of the party appeals theory, insofar as the parties may ultimately find themselves appealing with equal success to middle- and working-class groups. Our findings for England over a decade and a half produce partial, but not complete, support for this hypothesis. Although the Labour Party has been able substantially to overcome the losses from the gradual erosion of its traditional working-class base, it has not totally overcome the problem, and from the results in Figure 6, we see that Labour was still registering a small net loss in 1979 as the result of socioeconomic change within the electorate.

The notion of party adaptability should not be taken too far, however. There are, in fact, three practical factors that limit the ability of parties to transform their appeals along the lines of the Downsian model. First, if an appeal is aimed at the center, then the party risks losing support from its traditional supporters. To obviate this, the party may attempt to disguise the appeal and make it gradual, so as to retain the allegiance of its "natural" supporters. Second, party members and institutional interests may also oppose such a change of appeal because these may well remain committed to the party's basic principles on grounds of faith (or, in the case of groups such as trade unions or business, on grounds of self-interest). We can see this type of conflict in the almost perennial conflict between the more ideologically oriented left-wing Labour Party activists and their more moderate parliamentary leadership (Howell, 1979; Minkin, 1978). Third, voters tend not to choose a party *de novo,* and political socialization and the feelings of affective loyalties that voters have for one or other party impose a pervasive constraint on party successfully altering its appeal.

## Discussion

Although there can be little dispute that the importance of class in structuring the vote is declining in many advanced industrial democracies, it has been unclear how far this process has gone, what its nature is, and what its implications are for the electoral fortunes of the major political parties. In this article we have examined two theories, the decline-of-class theory and the party appeals theory, and postulated two pairs of hypotheses that speak directly to them. To test them we have used survey data from England spanning 15 years. Each theory was only partially supported. The decline-of-class theory rightly predicts that economic development will erode the working-class bases of left-wing parties (Hypothesis 1), but fails to see that parties may change their appeals to offset their consequent electoral disadvantage (Hypothesis 2). Similarly, the party-appeals theory isolates the importance of shifting appeals (Hypothesis 3), but fails to take into account that parties cannot change their appeals too rapidly or completely without risking the alienation of their traditional supporters (Hypothesis 4). In short, both theories accurately predict the *mechanisms* of change, but are less than accurate in recognizing the constraints that limit it.

Our findings show that had the class and party relationship in England remained as strong as it was in the early 1960s, the potential shifts resulting from economic growth were large and consistent enough to have greatly favored the Conservatives.[5] Indeed, changes in the level of education alone would have increased the Conservative's vote by more than 3% and changes in other class variables (class self-image, white-collar occupation, council housing, father in a white-collar occupation) by smaller amounts. Taken together, these shifts would have transformed British politics, burdening Labour with a built-in disadvantage that would have been sufficient to exclude it from power under all but the most exceptional circumstances. For example, the hypothetical Conservative socioeconomic gain of 3.5% is greater than the percentage gap between the two parties in four of the six general elections between 1964 and 1979.[6] From a theoretical perspective, scholars have been right to focus attention on the erosion of the left's natural class base (Lipset, 1981). Our span of only 15 years shows a 3.5% loss, but in, say, 30 years, that loss could be 7% and over two generations from the 1920s to the present it could be 14%. The clear implication is that economic growth can fundamentally alter the balance of power between left and right within even the time span of a generation.

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[5] Britain's much-publicized economic difficulties in the late 1970s and early 1980s are mainly a matter of *slower* long-term growth than in other European countries, not (save for a time during the worst of the recent depression) of stagnation or decline. They have fallen back relative to other countries but not in absolute terms, and it is the absolute, not the relative, changes that are central to the decline-of-class theory.

[6] The exceptions were 1966, when the gap was 6.2%, and 1979, when the gap was 7.0%.

As it turned out, however, Labour was able to alter the bases of its appeal so that the 1964 pattern did not continue into the late 1970s. By 1979 it was facing only a 1% actual disadvantage owing to socioeconomic changes other than housing, not the 3% hypothetical disadvantage. Moreover, the changes in father's partisanship and (to a lesser extent) trade unionization advantaged Labour by about 3% in 1979. The simple fact of the matter is that these are factors whose influence the Conservatives could not easiy negate; father's partisanship is an aspect of political socialization and not therefore subject to alteration through changing appeals or policies, while Labour's relationship with the unions is an enduring historical connection. But equally, Labour was unable to defuse the housing issue and lost heavily (nearly 3%) on that. The net result is that the Conservatives gained only 1% in 1979 from all the changes combined. We must, therefore, look elsewhere for the explanation of Labour's poor showing in the 1979 election and, by implication, of their disastrous performance in the 1983 election. As has been shown elsewhere, Labour's decline has more to do with territorial factors than with fundamental changes in the electorate's political attitudes (McAllister & Mughan, 1985; McAllister & Rose, 1984).

One final point is worthy of discussion. If, in line with the party-appeals theory, a party is theoretically able to shift its appeal to suit the nature and type of social groups present within the electorate, then it follows that parties will be seeking to appeal to the same clientele once the proportion of middle class and working class voters in the electorate become equal in size. Hence, class polarization will decrease, if not disappear altogether. But we can also take the logic of this further and suggest that if the middle class actually becomes numerically dominant, then it is in the interests of the *party of the right* to implement a direct class appeal. In both Australia and the United States the proportions of white- and blue-collar workers are almost equal, and it would appear that their respective parties of the right have, in recent years, been more prepared to aim their appeals more narrowly at the middle class than hitherto.

It is possible to make two further speculations should such a situation arise. First, there would be a change in the character of issues promoted by the party of the left; they would move away from class-related concerns and toward nonclass issues such as nuclear power, the environment, or consumer affairs. To some extent this change to issue rather than class appeals appears to have already begun in the United States (Nie, Verba & Petrocik, 1976; Pomper, 1972) and Britain (Budge, 1982; Crewe & Sarlvik, 1980; Robertson, 1984; Sarlvik & Crewe, 1983; Studlar, 1978; Studlar & Welch, 1981). Second, broadening the appeal of the party of the left (necessitated because the working class now constitutes a minority of electors) would provoke factional conflict, with the party activists on the left being deserted by the now moderate leadership. Equally, however, the moderate wing of the party of the right could desert the party as it intensifies its appeal to the middle class. Overall, we would predict that the consequences of economic growth would lead not only to a decline in class polarization, but to a shift in the kind of issues that are important to the left and to factional conflict as the left wings of both parties lose out to their respective center and right.

These predictions are, however, speculative. Our key argument is that the original "decline of class" theories, which predicted that economic growth would undermine parties of the left, were misplaced. Instead, through a variety of strategies these parties have been able to alter the basis of their appeal to the electorate on many, although not all, issues. While decreasing class polarization is the result of this process of adaptation, it does not signal the inevitable demise of working-class parties.

## Methodological Appendix

### Hypothetical Changes

Our basic model is presented in Figure 1, which gives the causal order among the variables. The results in Figures 3 and 4 are of two sorts, hypothetical and actual, and are obtained in different ways. The hypothetical changes are all based on regression analyses of voting behavior in the 1964 data. To get the effect of changes in the proportion of the electorate whose fathers were white-collar workers, for example, the following equation was estimated:

$$1964\,Vote = a + b_1\,FaWhiteC + b_2\,FaParty \qquad (1)$$

where $a$ is the regression constant, and $b_1$ and $b_2$ are metric partial regression coefficients. Note that this equation gives the total effect of having a white-collar father (including the parts that come about indirectly through the effects that it has on such factors as the respondent's own education, occupation, and union membership); variables reflecting respondent's own education, therefore do not appear in the equation (see Alwin & Hauser, 1975).

The predicted vote in 1964 can be obtained from equation (1) by multiplying the means for the independent variables by the regression coefficients and adding the products (e.g., Duncan, 1968). For example, in 1964 the proportion of the electorate who came from white-collar families was .20 and $b_1$ was −.13 (indicating that people from white-collar families were 13% less likely to vote Labour), so that part of the calculation would be:

$$1970\,Vote = a - (.13 \times .20) + b_2 FaParty \qquad (2)$$

But the proportion of the electorate from white-collar families is slowly increasing, rising to .23 by 1970. On the (hypothetical) assumption that the relation between these variables and the vote did not change between 1964 and 1970, the effect of this change on the vote can also be estimated from equation (1) by replacing the old proportion, .20, by the new, .23:

$$1964\,Vote = a - (.13 \times .23) + b_2 FaParty \qquad (3)$$

The difference between the predicted vote in equation (2) and that in equation (3) shows the change in the vote that would have come about because of the increase in voters from white-collar backgrounds. So, subtracting equation (3) from equation (2):

$$\begin{aligned} &\text{Hypothetical change} \\ &\quad = \text{equation (2) from equation (3)} \\ &\quad = b_1\,(1970\,FaWhiteC - 1964\,FaWhiteC) \\ &\quad = -.13 \times (.23 - .20) \\ &\quad = -.0039. \qquad (4) \end{aligned}$$

This result implies a drop in the Labour vote of nearly 0.4% between 1964 and 1970. Figures for 1974 and 1979 are obtained similarly, using the proportion of voters from white-collar backgrounds in those years.

Estimates for other variables are obtained similarly, replacing equation (1) with the appropriate equation giving their total effects. For example, the effect of home ownership is estimated from:

$$\begin{aligned} 1964\,Vote &= a' + b_1'\,FaWhiteC \\ &\quad + b_2'\,FaParty + b_3'\,Educ + b_4'\,WhiteC \\ &\quad + b_5'\,HomeOwner. \qquad (5) \end{aligned}$$

The results from these calculations are presented in Figures 3 and 4: The actual figures are in Table A2, where the calculations are line 2 times line 5 (for 1970), line 3 times line 5 (1974), and line 4 times line 5 (1979).

## Actual Changes

The estimates just described show what would have happened if there were changes in the sort of people in the electorate (e.g., an increase in the proportion coming from white collar families) but no changes in the *relation* between these variables and the vote since 1964 (e.g., no change in the 13% differential between white- and blue-collar voters). But in fact the relations changed. For example, by 1970 those born into white-collar families were not 13% but only 12% less likely to vote Labour. Thus the actual growth in the white-collar class was not so disadvantageous to Labour as it would have been if the 1964 relation between class and vote had remained unchanged.

The actual vote in 1970 is therefore not to be found from equation (1) but from its 1970 analog, estimated using 1970 (rather than 1964) data:

$$\begin{aligned} 1970\,Vote &= a'' + b_1''\,FaWhiteC \\ &\quad + b_2''\,FaParty. \qquad (6) \end{aligned}$$

The actual change in voting results from the increase in voters from white-collar families is analogous to equation (4):

$$\begin{aligned} &\text{Actual change} \\ &\quad = b_1''\,(1970\,FaWhiteC - 1964\,FaWhiteC) \\ &\quad = -.12 \times (.23 - .20) \\ &\quad = -.0036. \qquad (7) \end{aligned}$$

Estimates for other years are computed similarly. Estimates for other variables are computed from the analogs to equation (5) estimated for 1970 and later years. These results are reported in Figures 3 and 4. The actual data are in Table A2; the calculation is line 2 times line 6 (for 1970), line 3 times line 7 (for 1974), and line 4 times line 8 (for 1979).

## Changes from All Sources Combined

The actual result of all these changes combined is shown in Figure 5. It is not, however, simply the sum of the effects computed from equation (7) because there is some overlap in them. For example, the increase in the proportion of the electorate born into white-collar families affects voting mainly indirectly: children born to white-collar families are likely to get more education and better jobs, and to own their homes, and only because of those things to vote differently. Our estimates of the effects of home ownership include some of these changes (since the increase in home ownership is in part a consequence of the increase in white-collar families, for example). In order to avoid this kind of double counting, the estimate for the combined effect of all variables is taken from the regression equation giving the direct rather than the total effect of these variables (Table A2, lines 9 to 12). The effects for 1970, for example, are from the analog to equation (6), which includes all independent variables simultaneously (i.e., *FaWhiteC, FaParty, Educ,*

## Appendix

### Table A1. Correlations between Variables, 1964-1979[a]

| | Labour voter (1) | Father white collar (2) | Father Labour partisan (3) | White collar worker (4) | Trade union member (5) | Owner-occupied (6) | Class self-image (7) | Education (8) | Standard deviation (9) |
|---|---|---|---|---|---|---|---|---|---|
| 1. Labour voter | | | | | | | | | |
| 1964 | 1.00 | -.196 | .433 | -.294 | .211 | -.221 | -.373 | -.259 | .471 |
| 1970 | 1.00 | -.173 | .421 | -.254 | .216 | -.168 | -.289 | -.108 | .479 |
| 1974 | 1.00 | -.211 | .355 | -.274 | .219 | -.289 | -.282 | -.163 | .448 |
| 1979 | 1.00 | -.178 | .367 | -.226 | .206 | -.247 | -.259 | -.152 | .457 |
| 2. Father white collar | | | | | | | | | |
| 1964 | -.196 | 1.00 | -.211 | .278 | -.066 | .122 | .258 | .202 | .408 |
| 1970 | -.173 | 1.00 | -.184 | .340 | -.062 | .106 | .245 | .273 | .405 |
| 1974 | -.211 | 1.00 | -.213 | .299 | -.057 | .205 | .246 | .339 | .435 |
| 1979 | -.178 | 1.00 | -.285 | .290 | -.039 | .168 | .238 | .361 | .451 |
| 3. Father Labour partisan | | | | | | | | | |
| 1964 | .433 | -.211 | 1.00 | -.168 | .132 | -.127 | -.194 | -.109 | .381 |
| 1970 | .421 | -.184 | 1.00 | -.183 | .195 | -.144 | -.122 | -.099 | .434 |
| 1974 | .355 | -.213 | 1.00 | -.151 | .118 | -.134 | -.207 | -.127 | .420 |
| 1979 | .367 | -.285 | 1.00 | -.176 | .107 | -.177 | -.236 | -.183 | .426 |
| 4. White collar worker | | | | | | | | | |
| 1964 | -.294 | .278 | -.168 | 1.00 | -.098 | .240 | .388 | .388 | .475 |
| 1970 | -.255 | .340 | -.183 | 1.00 | -.121 | .205 | .347 | .255 | .474 |
| 1974 | -.274 | .299 | -.151 | 1.00 | -.073 | .299 | .280 | .374 | .490 |
| 1979 | -.226 | .290 | -.176 | 1.00 | -.051 | .287 | .367 | .371 | .492 |
| 5. Trade union member | | | | | | | | | |
| 1964 | .211 | -.066 | .132 | -.098 | 1.00 | -.052 | -.120 | -.068 | .414 |
| 1970 | .216 | -.062 | .195 | -.121 | 1.00 | -.058 | -.189 | -.036 | .438 |
| 1974 | .219 | -.057 | .118 | -.073 | 1.00 | -.021 | -.107 | .012 | .436 |
| 1979 | .206 | -.039 | .107 | -.051 | 1.00 | -.039 | -.100 | .005 | .462 |
| 6. Owner-occupier | | | | | | | | | |
| 1964 | -.221 | .122 | -.127 | .240 | -.052 | 1.00 | .201 | .183 | .489 |
| 1970 | -.168 | .106 | -.144 | .205 | -.058 | 1.00 | .247 | .142 | .500 |
| 1974 | -.289 | .205 | -.134 | .299 | -.021 | 1.00 | .223 | .229 | .496 |
| 1979 | -.247 | .168 | -.177 | .287 | -.039 | 1.00 | .258 | .228 | .487 |
| 7. Class self-image | | | | | | | | | |
| 1964 | -.373 | .258 | -.194 | .388 | -.120 | .201 | 1.00 | .387 | .459 |
| 1970 | -.289 | .245 | -.122 | .347 | -.189 | .247 | 1.00 | .231 | .482 |
| 1974 | -.282 | .246 | -.207 | .280 | -.107 | .223 | 1.00 | .266 | .474 |
| 1979 | -.259 | .238 | -.236 | .367 | -.100 | .258 | 1.00 | .309 | .475 |
| 8. Education | | | | | | | | | |
| 1964 | -.259 | .202 | -.109 | .388 | -.068 | .183 | .387 | 1.00 | 1.37 |
| 1970 | -.108 | .273 | -.099 | .255 | -.036 | .142 | .231 | 1.00 | 1.73 |
| 1974 | -.163 | .339 | -.127 | .374 | .012 | .229 | .266 | 1.00 | 1.35 |
| 1979 | -.152 | .361 | -.183 | .371 | .005 | .228 | .309 | 1.00 | 1.47 |

[a] Means are given in Table 1.

**Table A2. Means and Regressions, 1964-1979**

| | | | | Class Variables | | | | |
|---|---|---|---|---|---|---|---|---|
| | Father white collar[a] (1) | Father Labour partisan (2) | Trade union member (3) | Educa-tion[a] (4) | White collar worker[a] (5) | Home owner (6) | Class self-image[a] (7) | Variance explained ($R^2$) (8) |
| Means | | | | | | | | |
| 1. 1964 means | .199 | .514 | .219 | 9.81 | .367 | .396 | .319 | — |
| 2. Change, 1964-1970 | .031 | .048 | .040 | 0.22 | .023 | .129 | .014 | — |
| 3. Change, 1964-1974 | .052 | .048 | .036 | 0.36 | .038 | .170 | .024 | — |
| 4. Change, 1964-1979 | .078 | .048 | .090 | 0.55 | .057 | .219 | .036 | — |
| Estimated Total Effects (Metric partial regression coefficients) | | | | | | | | |
| 5. 1964 data | -.127* | .506* | .152* | -.169* | -.160* | -.105* | -.212* | — |
| 6. 1970 data | -.118* | .445* | .135* | -.012* | -.168* | -.072* | -.177* | — |
| 7. 1974 data | -.146* | .347* | .174* | -.028* | -.178* | -.174* | -.121* | — |
| 8. 1979 data | -.081* | .370* | .161* | -.022* | -.139* | -.141* | -.103* | — |
| Estimated Direct Effects (Metric partial regression coefficients) | | | | | | | | |
| 9. 1964 data | -.013 | .419* | .138* | -.028* | -.087* | -.090* | -.212* | (32%) |
| 10. 1970 data | -.026 | .389* | .107* | .003 | -.103* | -.044 | -.177* | (26%) |
| 11. 1974 data | -.045 | .276* | .164* | .001 | -.112* | -.161* | -.121* | (26%) |
| 12. 1979 data | -.019 | .301* | .153* | .001 | -.079* | -.129* | -.103* | (22%) |

[a]Means for these variables smoothed by linear interpolation for reasons given in the text.

*Significantly different from zero at $p < .05$, two-tailed.

*WhiteC, Homeowner,* and *Union*), estimated using 1970 data. The estimates for later years are from the same equation applied to data for the later year. The actual data are in Table A2. The calculation is line 2 times line 10, summed (for 1970); line 3 times line 11, summed (for 1974); and line 4 times line 12, summed (for 1979).

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# Party Goals and Government Performance in Parliamentary Democracies

KAARE STROM
*Michigan State University*
*University of Bergen*

*From assumptions of parties as rational actors, this study develops four measures of government performance: duration, mode of resignation, subsequent alternation, and electoral success. These measures are used in a test of competing hypotheses concerning minority government performance in parliamentary democracies. Minority governments are conventionally portrayed as poor performers, but tests of this proposition have been seriously limited. An alternative hypothesis depicts minority governments as rational cabinet solutions without significant performance liabilities. These hypotheses are tested against an extensive cross-national data set including 323 postwar governments in 15 parliamentary democracies. The conventional wisdom about minority governments is not supported by the evidence. In some respects, minority governments are clearly superior to majority coalitions. Moreover, minority government formation may enhance systemic responsiveness and accountability. The findings support the explanation of minority governments as rational cabinet solutions.*

## Parliamentary Government and Political Performance

The performance of democratic regimes and governments is a central concern in political science. Yet systematic efforts to study political performance cross-nationally have been rare until recently (Almond & Powell, 1978; Gurr & McClelland, 1971; Powell, 1982). In this analysis I will investigate the performance of various types of parliamentary governments. In so doing, I have two main objectives. The first is to bridge the traditional gap between the study of political performance and the more sophisticated analysis of government and coalition formation. Secondly, I derive performance measures that permit a test of competing hypotheses concerning government formation. More specifically, I compare the performance of minority governments (or cabinets) to that of single-party and coalition governments of majority size.

A minority government is any government formed by a set of parties that controls fewer than half the seats in the national legislature (or in bicameral legislatures: the lower house). The conventional wisdom portrays such governments as highly unviable and ineffective cabinet solutions. The mere absence of a governmental majority is commonly taken as an indication of cabinet ineffectiveness (Powell, 1981, p. 869; 1982). Johnson's (1975) summary of the disadvantages of minority governments aptly represents many of the misgivings frequently expressed. In Johnson's (1975, p. 87) view, minority governments tend to be:

1. Weak and exposed to the risk of parliamentary defeat,
2. Opportunistic and in search of ways to escape their minority position, and
3. Lacking in the authority and support necessary to handle serious problems.

Without addressing these specific propositions, this article will test the conventional view of minority cabinet performance. Contrary to the conventional wisdom, I hypothesize that minority governments generally perform as well as their natural competitors, that is, majority coalitions. I have argued elsewhere that minority governments emerge as rational cabinet solutions (Strom, 1984b). According to this explanation minority cabinets form when the benefits of holding office are outweighed by the costs for a majority segment of the party system. The benefits of governing are inversely related to the opportunities for opposition parties to influence parliamentary legislation. The costs of holding office are greater the more competitive and decisive future elections are likely to be. Consequently, minority governments should form when both *oppositional influ-*

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*ence* and *electoral competitiveness (or decisiveness)* are high. If minority governments are indeed rational cabinet solutions, then their performance should not be systematically inferior to that of competing cabinet types. On the other hand, if minority governments are in fact feeble performers, then the rationalist explanation is weakened. In the latter case one would have to assume that party leaders attach significantly greater utility to participation in majority governments than to inclusion in minority cabinets. The contention that these leaders might rationally prefer minority governments under certain conditions would then be much less credible. Thus, the analysis that follows will serve as an additional and independent test of the latter theory.

But the analysis of government performance can only serve as such a test if it meets three conditions. First, the performance measures will have to be relevant to the calculations of party leaders in the game of government formation. Invoking rationality in this context implies that government performance affects the expected utilities of party leaders, and the performance measures have to render this assumption plausible. Second, the two competing hypotheses concerning government performance must be tested extensively against a large set of cross-national data. Finally, one must control for whether or not potential performance differentials are due to underlying structural conditions. For example, minority governments might perform poorly because they form in situations where no government could expect to be successful. If so, it would be wrong to take their occurrence as evidence against an explanation based on rationality. In brief, the three conditions above spell out the tasks of the empirical analysis.

## Political Performance: Functional Approaches

Valid and theoretically interesting measures of government performance are in short supply. Eckstein (1971) has pointed out some of the problems inherent in the conceptualization and measurement of political performance. It is difficult to find criteria of evaluation that are neither overly broad and vague (e.g., system maintenance) nor excessively specific and arbitrary (e.g., allocations of government spending). In the former case, causal relationships between the relevant variables may be rather complex and indirect. In the latter case, there may be considerable controversy within and between political systems over the values on which we base our judgments.

Most authoritative studies of political performance have been explicitly or implicitly functionalist in orientation. However, few have claimed to have formulated grand theory or specified requisite functions. Within the functionalist framework, seminal works have focused on such general variables as civil order, participation, legitimacy, and regime stability (Eckstein, 1971; Powell, 1982). The concern has been with performance as systemic outcome, but these outcomes may have as much to do with environmental conditions as with internal conversion processes. Thus, the causal link between the properties of political institutions (e.g., governments) and such outcomes as legitimacy and civil order may be highly tenuous. Attempts to conceptualize government performance have yielded similarly broad and unspecific categories (Blondel, 1968; Finer, 1975; Spiro, 1959).

The opposite extreme in functional analysis of government performance is a focus on subsystemic output. One might evaluate cabinets on the basis of parliamentary appropriations for public services, on the level of tax extraction, or on the volume of parliamentary legislation (Almond & Powell, 1978). The validity of such performance measures is open to serious challenge (Di Palma, 1977, pp. 7-27). If output measures are selected subjectively, we almost inevitably involve ourselves in value judgments over which political leaders may be fundamentally at odds. Some politicians strive to increase the size of the public sector; for others, the exact opposite is a primary objective. It is hardly the task for empirical political scientists to settle such disputes a priori.

## Government Performance: A Rational Choice Approach

As an alternative to the functionalist approach, I link the analysis of government performance to the objectives of government leaders. This strategy has its own problems. For one thing, governments may vary widely in their ambitions. A typical caretaker administration may seek only routine administration for a few months until a new election is held. Other governments desire thoroughgoing changes in the political and economic structures of their countries (Dahl, 1966, pp. 341-344). It would obviously be misleading to consider these goals equivalent in an evaluation of cabinet performance. Moreover, cabinets may be more or less explicit in their formulation of goals, and their goals may be conditioned by the political environment. Consequently, comparisons between and even within countries become problematic.

Yet a useful specification of performance dimensions need not elude us. Based on rational choice assumptions, performance measures can be derived deductively from party goals. In so doing, I assume that parliamentary parties (or their leaders) act as cohesive units, and that these parties pursue a variety of goals in the cabinet formation game. Furthermore, whether in government or opposition, party leaders are assumed to be motivated primarily by partisan rather than systemic considerations. Under these assumptions, government performance can be identified as the performance of the governing parties.

Substantively, I distinguish between policy influence and political office as party goals (see Wittman, 1983). In this context, "political office" refers to participation in government. A major reason that parties wish to be in office lies in their interest in parliamentary influence, but this is hardly the only reason. The spoils of office count for something in their own right, and government participation may also have a certain symbolic value for political parties and their leaders. Conversely, one need not participate in the cabinet in order to enjoy some degree of legislative influence, although officeholding is certainly conducive to political effectiveness (Strom, 1984b).

Party objectives are also complicated by temporal perspectives. Political parties have immediate as well as longer-term goals, and tradeoffs may be necessary along the temporal dimension as well as between office and influence. I distinguish between short-range and medium-range party goals. In practical terms, the distinction between the short and the medium term corresponds to the difference between pre-electoral and postelectoral considerations. The next election is the first occasion when resources (votes) can be substantially reallocated between the parties. To the extent that elections are competitive and decisive for cabinet formation, conflicts may arise between any party's pre-electoral and postelectoral goals. This is because the voters will react to the government participation choices of the various parties. Governing now may reduce the chances of office as well as influence after the next election (Downs, 1957; Rose & Mackie, 1983).

Clearly, most parties have goals that go beyond the next electoral period. However, in the following analysis I disregard the longer term for three reasons. One reason is the great uncertainty attached to predictions of the parliamentary situation several elections ahead. The second is the limited time horizons of party leaders as individuals. To the extent that personal interest enters into the calculations of party leaders, they are unlikely to be moved by opportunities in the distant future. The political durability of party leaders is short, and their ambitions must typically be realized soon or never. Thirdly, party leaders are constrained by the urgency and irreversibility of many policy decisions, which can be made only now and once. For these reasons, party leaders are likely to have steep discount rates for longer-term benefits.

In sum, parties will value officeholding before as well as after the next election. Apart from that, they will also wish to be influential in the future as well as today. Combining the substantive and temporal dimensions produces a typology of party goals in the cabinet formation game, as presented in Table 1. This typology provides us with a framework for analysis of government performance. I shall proceed to construct performance measures, which have been included in Table 1. Their purpose is to enable us to evaluate the degree to which various types of governments further the goals of the participating parties.

The performance measures have been designed to meet the conditions of relevance to the calculations of party leaders. I have also sought to capture dimensions of cabinet performance that are related to important constitutional functions of governments. This study therefore bears some indirect relation to functional analyses of government performance. The variables I present at least tap Eckstein's criteria of durability and decisional efficacy (Eckstein, 1971). Thus the performance variables indirectly indicate how well various types of parliamentary governments perform their constitutional functions.

## The Data Base

Government performance will be tested against a set of data including all recent governments in a number of parliamentary democracies with a significant record of minority governments. For the sake of the plausibility of the assumptions about party objectives, the scope of this study is

**Table 1. Typology of Party Goals**

| | Short range | Medium range |
|---|---|---|
| Office | Pre-electoral office<br>(Durability) | Post-electoral office<br>(Alternation) |
| Influence | Pre-electoral influence<br>(Resignation) | Post-electoral influence<br>(Electoral success) |

**Table 2. Governments by Country and Format, 1945-1982**

| | Majority | Minority N | Minority %$^a$ | Nonpartisan | Total | Mean parliamentary basis % |
|---|---|---|---|---|---|---|
| Belgium | 25 | 4 | 14 | 0 | 29 | 62.0 |
| Canada | 7 | 8 | 53 | 0 | 15 | 53.1 |
| Denmark | 3 | 20 | 87 | 0 | 23 | 40.1 |
| Finland | 19 | 11 | 30 | 7 | 37 | 54.6 |
| France | 18 | 12 | 40 | 0 | 30 | 51.0 |
| Iceland | 13 | 4 | 24 | 0 | 17 | 51.5 |
| Ireland | 10 | 5 | 33 | 0 | 15 | 50.5 |
| Israel | 23 | 2 | 8 | 0 | 25 | 62.0 |
| Italy | 26 | 19 | 42 | 0 | 45 | 51.8 |
| Netherlands | 15 | 3 | 17 | 0 | 18 | 61.6 |
| Norway | 8 | 10 | 56 | 0 | 18 | 47.4 |
| Portugal | 7 | 1 | 8 | 4 | 12 | 64.5 |
| Spain | 1 | 3 | 75 | 0 | 4 | 50.0 |
| Sweden | 9 | 10 | 53 | 0 | 19 | 47.2 |
| United Kingdom | 14 | 2 | 13 | 0 | 16 | 52.7 |
| Total | 198 | 114 | 35.3 | 11 | 323 | 53.2 |

$^a$Minority governments as a percentage of total governments.

Sources. Keesing's Contemporary Archives, Mackie and Rose (1982), Cook and Paxton (1975), *Facts on File.*

restricted to stable, liberal, and developed nations. The sample includes all advanced Western parliamentary democracies with a history of at least one minority cabinet per 20 years since 1945. For each country, all governments formed from May 1945 through 1982 are included.[1] I have counted governments according to the generally accepted practice for each country,[2] except that any of the following conditions is sufficient to constitute a change of government:

1. Change of prime minister, or
2. Change of parties represented in the cabinet, or
3. General elections, or
4. By-elections resulting in a change in the government's parliamentary basis from majority to minority status, or vice versa (see Lijphart, 1982).

These parameters yield a total of 323 governments in the following 15 countries: Belgium, Canada, Denmark, Finland, France, Iceland, the Republic of Ireland, Israel, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and the United Kingdom. For all intents and purposes, this sample constitutes the entire universe of post-1945 minority governments.[3] Table 2 presents a breakdown of the governments included. Note that the overall frequency of minority governments is as high as 114 out of 323, or 35.3%, which corresponds to close to eight minority cabinets per country. Whether in absolute or relative terms, this record can only be described as very substantial. In Canada, Denmark, Norway, Spain, and Sweden, more than half of all governments have been of minority status.

Among majority administrations, I distinguish between coalitions and single-party (majority party) governments. The latter tend to form whenever possible, that is, in every majority situation. In minority situations, when no majority party government is possible, the choice will normally be between a majority coalition and a minority government. The sample contains 42 majority party and 156 majority coalition governments. Finally, there is a small number of nonpartisan administrations: seven in Finland and four in Portugal. These are governments without any identifiable partisan support, mostly caretaker business administrations. Owing to their lack of partisanship, these governments cannot be assigned any value for parliamentary basis, that is, the percentage of legislative seats controlled by the governing parties.

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[1] Many political systems experienced all-party governments during and immediately after World War II. I have not included such governments in this analysis, even in cases where they were formed in the aftermath of the war. In other words, for each country the record begins with the first partisan parliamentary cabinet formed after the cessation of hostilities in Europe.

[2] The one significant problem in this respect involves governments, particularly in France and Italy, which fail their vote of investiture. In such cases, the most common practice is to include them, which has also been my choice here.

[3] In Europe, the only minoritarian case excluded is the Austrian Kreisky government of 1970-1971. Obviously, not all 15 nations have had continuous histories of parliamentary democracy since 1945. In such cases, the time span covered is shorter. Thus, the Israeli record begins in 1949, and the French ends with the demise of the Fourth Republic in 1958. The first Portuguese government included is the first Goncalves cabinet of 1975, and the first Spanish government the one formed by Suarez in July 1977.

### External Support

Some minority governments may be majority governments in disguise: governments with an equally secure, but less visible, basis in the assembly (Daalder, 1971, p. 288; De Swaan, 1973). Minority governments often have formal or informal support agreements with parties not represented in the cabinet. If the commitment of these external supporters is as strong as that of the governing parties, there would be no reason to expect the performance of minority governments to differ from majority coalitions. I have attempted to identify externally supported cabinets in this stricter and more interesting sense. Thus, a government is classified as externally supported if:

1. Its support is negotiated before the formation of the government, and
2. That support is an explicit, comprehensive, and more than short-term commitment to the policies as well as the survival of the government.

By these criteria, 35 of the governments in this study are externally supported. However, some of the externally supported cabinets are of majority size to begin with, and in other cases the inclusion of external support is not sufficient to cross the majority threshold. For only 13 governments does external support make the difference between minority and majority status. I call these formal minority governments. The remaining 101 minority cabinets will be referred to as substantive minority governments (Strom, 1984b).

## Pre-Electoral Office Performance

When parties decide to participate in government formation, they are presumably interested in some measure of government longevity. The point follows directly from my assumptions. If parties, ceteris paribus, seek to maximize pre-electoral officeholding, then they in effect wish to hold office for as long as possible before elections. Governing parties will seek to maximize government durability, that is, the longevity of their own rule. To use durability as a measure of government performance is not a novel idea (e.g., Blondel, 1980; Warwick, 1979), but in this analysis, durability taps one specific and theoretically derived dimension of government performance. Durability is measured as the tenure of each government in months.[4] It is common to associate minority governments with low levels of durability (Dodd, 1976, pp. 170-181; Lijphart, 1982; Warwick, 1979). Likewise, coalition governments have traditionally been perceived as less durable than single-party administrations, although this proposition has been challenged by Dodd (1976). Table 3 presents mean durability figures for the various cabinet types.

At first sight, the findings are fairly consistent with the conventional view of minority governments. Although majority governments enjoy an average duration of approximately 19.3 months, undersized cabinets tend to last only 13.8 months, and nonpartisan administrations no more than 4.1. When the majority and minority categories are subdivided, however, a more interesting pattern emerges. Table 3 shows us that there are two outlying categories. Majority party governments tend to be unusually durable, whereas nonpartisan administrations are very short-lived. The difference between majority coalitions and minority governments remains in favor of the former, but the contrast is far from dramatic. In fact,

**Table 3. Cabinet Duration by Cabinet Type**

| Cabinet Type | Mean Duration (months) |
|---|---|
| Majority party | 28.8 |
| Majority coalition | 16.6 |
| Minority formal | 13.5 |
| Minority substantive | 13.9 |
| Nonpartisan | 4.1 |
| All governments | 16.7 |

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[4] Duration of tenure has been measured from the date of formal investiture to the date when the prime minister submits his or her (subsequently accepted) resignation. In cases where the government continues in office after general elections, the date of elections has been used as a termination point. The duration figures have been rounded off to the nearest whole number of months.

cabinet composition (coalition vs. single-party) is more strongly related to cabinet durability than is numerical status. Whereas coalition governments survive for an average of 14.9 months, single-party cabinets endure for 21.1 months. These results may be a greater source of concern for Dodd's argument than for mine.[5] The durability advantage of majority party governments over majority coalitions is more than a year on average, that of the latter over substantive minority governments only 2.7 months. And for both of the latter two cabinet types, the range of variation is quite wide, with standard deviations of 13.8 and 12.7 months, respectively. Clearly, minority governments are nowhere near as transitory as nonpartisan administrations. Instead, their pre-electoral office performance does not drastically differ from majority coalitions, and there is no meaningful difference between formal and substantive minority governments.

### Pre-Electoral Influence Performance

Durable governments do not necessarily enjoy a great deal of legislative success. In fact, the contrary may be the case, in that inoffensive and ineffectual administrations may be tolerated for a longer period of time. Pre-electoral influence for the governing parties is not a simple function of cabinet longevity. More direct measures of policy effectiveness are clearly desirable. But cross-national analysis of legislative effectiveness is extremely problematic. Effectiveness is greatly constrained by a variety of institutional characteristics, for which we cannot hope to control in a satisfactory manner across 15 countries (Di Palma, 1977). Hence, this analysis will use a more indirect measure. Legislative effectiveness requires a degree of cohesion among the governing parties. The unity, and consequently the influence, of any government are likely to be reflected in the circumstances under which it resigns. Thus, the mode of resignation will serve as my measure of pre-electoral influence.

I shall distinguish between the following modes of resignation:

1. *Defeat-Crisis,* when the government is forced to resign, or does so in the face of some perceived adversity.
2. *Voluntary,* in cases where the government is under no substantial pressure to resign, but does so in order to broaden its parliamentary base, reshuffle personnel, or for other reasons.
3. *Technical,* that is, resignations that are neither voluntary nor related to any governmental defeat or crisis. Technical resignations tend to be a function of my rules of counting governments, as in the case when a prime minister dies or a government goes through a general election without any change in premiership, partisan composition, or numerical status (i.e., majority versus minority basis).[6]

Let us first look at the significance of various modes of resignation and then at the available data. The governing parties seek to minimize the severity of their resignations, and I reason that the more severe the resignation, the less their legislative influence. The modes of resignation have been listed in declining order of severity. A crisis-induced resignation more typically reflects a governmental effectiveness problem than does a technical resignation. Voluntary resignations form a more ambiguous intermediate category. On the one hand, it would be far-fetched to interpret the resignation of Danish premier Jens Otto Krag in October 1972, or that of Winston Churchill in April 1955, as evidence of government ineffectiveness. But the voluntary resignations of Italian prime ministers Giovanni Leone and Mariano Rumor, in November 1968 and February 1970 respectively, were just as clearly indications that their administrations lacked any long-term effectiveness or viability. Governments may resign voluntarily out of weakness or out of strength. Despite this ambiguity of voluntary resignations, I feel confident that the category overall represents an intermediate level of severity, between the defeat-crisis and technical modes.

Table 4 presents a breakdown of resignations by cabinet type. The conventional wisdom would lead us to believe that minority governments are inferior in pre-electoral influence. The argument that minority governments are ineffective is as common as the claim that they are short-lived (Dodd, 1976, p. 179; Lepsius, 1978, pp. 43-44; Linz, 1978, p. 66; Von Beyme, 1970, pp. 570-571). The results demonstrate that majority party governments resign under substantially more favorable conditions than any of the other types. Less than half of these resignations were pre-

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[5] However, I have not sought to test Dodd's theory, because there is no distinction in the data between minimum-winning and oversized coalitions.

[6] General elections have been counted as technical resignations only in cases of continuity of all three factors mentioned above: premiership, partisanship, and numerical status. In all other cases, resignations after general elections have been considered caused by defeats or crises.

**Table 4. Mode of Resignation by Cabinet Type (%)[a]**

| Cabinet Type | Defeat/Crisis | Voluntary | Technical |
|---|---|---|---|
| Majority party | 48 | 18 | 33 |
| Majority coalition | 85 | 5 | 10 |
| Minority formal | 64 | 36 | 0 |
| Minority substantive | 67 | 16 | 16 |
| Nonpartisan | 82 | 18 | 0 |
| All governments | 74 | 12 | 14 |

[a]The percentages in each row do not necessarily add up to 100 because of rounding.

cipitated by a defeat or crisis, and fully a third were technical in nature. In contrast, no formal minority or nonpartisan administration is on record with a technical resignation, although it should be kept in mind that these cabinet types are few in number.

More important, the profile for substantive minority governments is significantly more flattering than that for majority coalitions. The relative frequency of voluntary and technical resignations combined is more than twice as high for the former cabinet type. Substantive minority governments are thus superior to majority coalitions, although inferior to majority party governments, in performance relevant to their pre-electoral influence goals. A separate analysis of causes of governments resignation (not reported here), using a sixfold classification, yields very similar results. Majority party governments resign due to the least severe causes, and the profile of substantive minority governments is significantly more favorable than that of majority coalitions (Strom, 1984a, pp. 165-168).

## Post-Electoral Office Performance

Governing parties seek to maintain their hold on executive office into future administrations and electoral periods. Hence, governments can be evaluated on the basis of how well they maintain themselves in office. If two successive governments are identical in terms of partisan composition, then the postelectoral office goals of the governing parties of the earlier cabinet have been served rather nicely. Consequently, my measure of postelectoral performance is subsequent alternation or turnover in government. The operational measure reflects the percentage of parliamentary seats held by parties changing status between government and opposition in a given change of government.[7] A hypothetical example may clarify the computation of alternation. In a given legislature, party A has 50% of the seats, party B 30%, and party C 20%. Party A initially governs alone. If the next government consists of parties B and C, then all parties will have changed status from the initial government, and the alternation score for the first government will be: 50 + 30 + 20 = 100. If instead parties A and C form a coalition, then only C will have changed status, and the alternation score for the initial government will be 20.

The lower the alternation score, the better the postelectoral office goals of the governing parties are served. Alternation is measured at the next change of government. Thus, this variable reflects postgovernmental (but not necessarily postelectoral) turnover. In practice the difference between postelectoral and postgovernmental alternation is unlikely to be dramatic. In a majority of the 15 countries, postelectoral and postgovernmental alternation coincide in more than three-fifths of the cases. Hence, postgovernmental alternation is also a fair indicator of post-electoral alternation.

Subsequent alternation is greater for minority governments than for those of majority size. The mean alternation scores for these two broad categories are 28.3 versus 19.9. Table 5 demonstrates that subsequent alternation is also significantly higher for substantive minority governments than for majority coalitions. Thus, from the point of view of the participating parties, majority coalitions perform better than minority governments. But some curious aspects of the findings should be mentioned. It is, for example, interesting to note the relatively high score of majority party governments and the very low

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[7] Our indicator for alternation is computed as the sum of the percentage of parliamentary seats held by parties leaving government and the percentage of seats held by parties entering government. Where general elections intervene between two governments, all calculations have been made on the basis of the composition of the legislature *after* the election. For technical reasons, the maximum alternation score is 98.

**Table 5. Subsequent Alternation by Cabinet Type[a]**

| Cabinet Type | Full Sample | Nontechnical Resignations Only |
|---|---|---|
| Majority party | 21.7 | 32.8 |
| Majority coalition | 18.9 | 21.3 |
| Minority formal | 11.2 | 11.2 |
| Minority substantive | 30.3 | 34.6 |
| All cabinet types | 23.0 | 26.4 |
| *N* | 289 | 245 |

[a]Alternation figures represent percentages of legislative seats involved in cabinet turnover.

score of formal minority governments. These tendencies are at odds with the results on other performance measures, where majority party governments consistently perform very well and formal minority cabinets rather poorly.

The second important point is that the range of variation is much higher for majority governments, and that the range is similarly greater for substantive minority governments than for majority coalitions. The results suggest that majority party and substantive minority governments tend to form in systems of alternative government, where turnover in office is likely to be an all-or-nothing proposition. In the Westminster model of democracy, elections either result in a complete change of government, or no change at all (Lijphart, 1984, pp. 4-9). In order to test for this possibility, I have also run the analysis excluding those governments whose resignations were technical (see above). This procedure permits a focus on those cases where alternation is a realistic possibility. As Table 5 demonstrates, the alternation score for majority party governments increases dramatically under these conditions, and there is also a noticeable increase for substantive minority governments. The convergence of these two cabinet types is again evident, and the contrast to majority coalitions more substantial. The high alternation scores of these cabinets types should be less satisfactory than those of majority coalitions for the participating parties. Whether a low degree of alternation is similarly desirable for the political system as a whole is an entirely different question.

## Post-Electoral Influence Performance

Post-electoral influence is of course in large part a function of whether governing parties gain entry to subsequent governments, but given the distinction between office and influence performance, the latter must be measured without reference to government participation. So the question is: Given an institutional framework, what makes parties more or less influential, irrespective of cabinet participation? The essential determinant here is electoral fortunes, in that a large party will tend to be more influential than a small one, and a recently successful party more influential than one with a history of recent electoral setbacks. Hence, postelectoral influence will be measured as subsequent electoral success.

Beyond its usefulness as an indicator of influence, electoral success is a fundamental party objective pursued for a variety of reasons. By Sartori's definition, only electoral goals are necessarily common to all political parties (Sartori, 1976, p. 63), and considerations of future electoral fortunes are a critical part of explanations of party strategy in general and minority government formation in particular (Bueno de Mesquita, 1975; Downs, 1957; Strom, 1984b). Electoral success is therefore a crucial aspect of government performance.

Electoral success can be operationalized in various ways. One may count electoral gains and losses in either parliamentary seats or votes. For coalition governments, the analysis could also be focused either on the overall gains or losses of the coalition or on the fate of the individual parties. The measure here is the aggregate changes in popular vote (in percentage terms) for each government in the next general election after its formation.[8] The findings corroborate the argument that incumbency tends to be an electoral liability (Rose & Mackie, 1983). Table 6 demon-

**Table 6. Mean Aggregate Electoral Success by Cabinet Type[a]**

| Cabinet Type | Full Sample | Pre-Electoral Governments Only |
|---|---|---|
| Majority party | −2.45 | −2.29 |
| Majority coalition | −4.53 | −4.05 |
| Minority formal | 0.13 | −0.40 |
| Minority substantive | −1.44 | −1.17 |
| All cabinet types | −3.05 | −2.61 |
| *N* | 289 | 107 |

[a]Percentage points of total vote.

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[8] Very similar results were obtained when parties, rather than governments, were used as units of analysis. Not only do majority coalitions in aggregate suffer greater losses than minority governments, but the proportion of gaining parties within majority coalitions is also much lower (Strom, 1984a, pp. 177-178).

strates that the liability of incumbency is shared by most cabinet types. Only the small class of formal minority governments show a miniscule net gain. Yet, electoral incumbency costs are far from equally shared by the three major cabinet types. Majority cabinets lose an average of 4.09% of the total vote, minority administrations no more than 1.28%. The average majority coalition loses almost twice as much as majority party governments, and more than three times as much as substantive minority cabinets.

Relative to their former electoral basis, majority coalitions suffer an average loss of 7.2% of their support. The corresponding figures for majority party and substantive minority governments are 4.5% and 2.1%, respectively. Because majority coalitions tend to have broader initial support, their electoral losses are slightly less dramatic in relative than in absolute terms. Yet an average swing of more than 4% of the total vote away from the governing parties must be considered substantial, and the anticipation of such a hemorrhage may well be an effective deterrent to majority coalition formation.

Electoral success or failure can most meaningfully be attributed to governments in office during general elections. The second column of Table 6 presents a breakdown of electoral success confined to governments in office at election time. As we can see, this restriction has little impact on the pattern described above. All cabinet types, with the exception of formal minority governments, fare a little better when they are in office during elections. But none of these improvements is dramatic, and they are fairly parallel for the various cabinet types.

In sum, the figures in Table 6 yield a consistent picture of the electoral success of various types of governments. Majority coalitions perform considerably worse in subsequent elections than majority party governments. The latter are in turn inferior to minority governments. Thus, on a key performance variable, we find minority governments to have a clear advantage over their competitors.

## Multivariate Analysis of Government Performance

We have explored the performance of various cabinet types on a number of dimensions. However, the analysis has remained bivariate, and no attempt has been made to trace the observed differences back to dissimilarities in the conditions under which various cabinets form. In the following analysis, I introduce a variety of control variables to investigate whether or not these can account for the performance differences between majority coalitions and substantive minority governments.

Not all performance measures lend themselves to this type of study. The analysis will be confined to interval-level variables, because only these meet the assumptions of standard regression analysis. Thus, this part of the study will include duration, alternation, and electoral success. The analysis will be restricted to minority situations with partisan outcomes, which is to say that majority party and nonpartisan governments will be excluded. Majority party governments are excluded because they can form only in majority situations, in which minority solutions are not feasible and would not be predicted by any theory. Nonpartisan administrations are left out because no value can be assigned to them on the crucial variable of parliamentary basis. I shall use two distinct indicators of parliamentary basis: one continuous and one dichotomous. The continuous measure (parliamentary basis) is as earlier the percentage of parliamentary seats held by the governing parties. External support will be considered part of the parliamentary basis. Secondly, a dummy variable (numerical status) will distinguish between majority coalitions and substantive minority governments. This variable is coded 0 for substantive minority governments and 1 for majority coalitions and formal minority governments. The latter procedure allows us to get a direct measure of the impact of the difference between majority and minority governments.

The multivariate analysis will build on previous efforts to explain the formation of minority versus majority governments. Minority government formation is related to a variety of structural and proximate conditions, and I have argued that such governments can be explained as rational cabinet solutions under specified conditions (Strom, 1984b). The following analysis includes the variables that have been found to affect minority government formation. The main results of that analysis are presented in Figures 1 through 3. The general hypothesis here is that variables that affect the formation of minority government might also have an impact on their performance. These variables include party system fractionalization (Rae, 1971, pp. 53-58), polarization or extremist party support (Powell, 1982, pp. 92-96, 233-234), electoral volatility for parliamentary seats (Pedersen, 1979), the policy influence of the parliamentary opposition (Strom, 1984a, pp. 121-124),[9] the salience of general elections for government formation, the *responsiveness* of government formation to electoral verdicts,[10] the spatial *concentration of the parliamentary opposition* (Dahl, 1966, pp. 332-336) on either the left or right side of the government,[11] the existence or nonexistence of legislative *investiture requirements,* the *duration of the crisis* preceding each government formation (in days), and the *number of formation attempts* during these crises.

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[9] This variable is a five-point scale based on characteristics of the legislative committee systems. The more specialized and autonomous the legislative committees

Most of these variables are conventional measures or have previously been introduced in this analysis. Others are discussed in the footnotes. However, the complex and powerful measure of electoral salience deserves a few words of explanation. Electoral salience is a composite indicator based on 1) the degree of association between general elections and government formations (proximity), and 2) the identifiability of pre-electoral governmental alternatives. Each country is scored on a decade-to-decade basis, and each government assigned the mean value for the country and decade in which it was formed. A high degree of association between the two components has been established through factor analysis (Strom, 1984a, pp. 124-127).[12] Electoral salience, the influence of the opposition, volatility, and responsiveness are the main explanatory variables in the rational choice theory of minority government formation. Each is hypothesized to covary negatively with parliamentary basis.

I have used an ordinary least squares regression technique with a backward elimination procedure. Through previous analysis (Strom, 1984b), I can also determine indirect effects on government performance through parliamentary basis. Such indirect effects will be included in causal path models for each of the three interval-level performance measures. For technical reasons, this path analysis can only be based on the continuous measure of parliamentary basis. However, the path models will be supplemented with a presentation of the regression coefficients for models using the simple dichotomy between majority and minority governments (numerical status) as the crucial predictor. These two procedures should complement each other well. The former offers a comprehensive view of the factors that directly or indirectly affect government performance; the latter yields the most precise estimates of the performance differential between majority coalitions and minority governments.

## Cabinet Duration

Table 7 presents the regression analylsis for cabinet duration, including the full equation, with

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of a given nation, the higher its score on oppositional influence.

[10] Responsiveness has been measured for each country and decade as the proportion of parties entering government that had increased their share of parliamentary seats in the most recent general elections.

[11] Concentration is measured as the number of seats held by parties on the numerically largest opposition side as a proportion of all opposition seats.

[12] Proximity is operationalized as the proportion of all government formation that occur immediately follow-

**Table 7. Multivariate Analysis of Cabinet Duration**

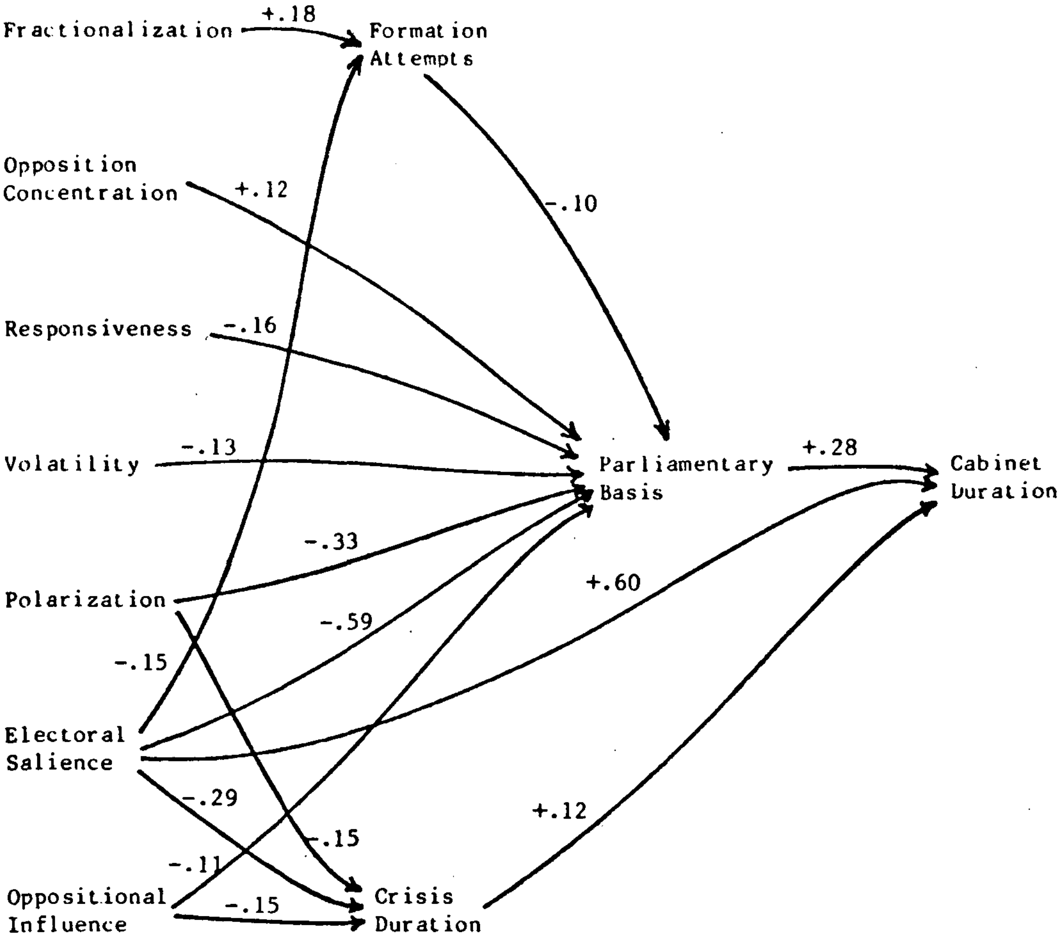
| | Full Equation | | | Final Equation | | |
|---|---|---|---|---|---|---|
| Variable | Coefficient | SE | Beta | Coefficient | SE | Beta |
| Numerical status | 6.89** | 1.71 | .25 | 7.72** | 1.56 | .28 |
| Influence of opposition | –.48 | .88 | –.03 | | NS | |
| Electoral salience | 8.13** | 1.41 | .53 | 9.34** | .87 | .60 |
| Volatility | 1.26 | 12.40 | –.01 | | NS | |
| Responsiveness | –.47 | 3.89 | –.01 | | NS | |
| Polarization | –.07 | .10 | –.06 | | NS | |
| Fractionalization | 3.49 | 15.59 | .02 | | NS | |
| Crisis duration | .05* | .02 | .13 | .04* | .02 | .12 |
| Formation attempts | –.62 | .53 | –.07 | | NS | |
| Opposition concentration | 2.74 | 5.84 | .03 | | NS | |
| Investiture | –.02 | 1.67 | –.01 | | NS | |
| Adjusted $R^2$ | | .305 | | | .319 | |

$N = 251$

NS = Not significant at .05 level

*Significant at .05 level
**Significant at .01 level.

Figure 1. Causal Path Model of Cabinet Duration


Fractionalization
+.18
Formation Attempts
Opposition Concentration
+.12
-.10
Responsiveness
-.16
Volatility
-.13
Parliamentary Basis
+.28
Cabinet Duration
-.33
Polarization
-.59
+.60
-.15
Electoral Salience
-.29
+.12
-.15
-.11
Oppositional Influence
-.15
Crisis Duration

Note: Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

all independent variables included irrespective of significance, as well as the final equation including only significant relationships. Obviously, few of the control variables have any meaningful impact on government durability. In fact, only three relationships meet the .05 level of significance. One of these is numerical status, that is, the distinction between majority coalitions and substantive minority governments. Depending on how many other variables are included in the analysis, majority size increases the longevity of parliamentary governments by approximately seven months. This is a substantial difference consistent with the bivariate analysis above.

However, in the path model the impact of electoral salience is considerably stronger than that of parliamentary basis. The beta weight of the former variable is .60, as against .28 for the latter. The more salient elections are for government formation, the longer governments endure. This variable alone accounts for a predicted difference between its extreme values of more than two years in cabinet duration.[13] It is no surprise that elec-

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ing general elections, measured for each country and decade. Identifiability has been scored impressionistically as low (0), medium (0.5), or high (1) for each election. The mean has then been computed for each nation and decade.

[13] The observed range for electoral salience is approximately 3.1 points.

toral salience is related to cabinet durability. After all, the former variable measures the extent to which changes of government are linked to the electoral cycle, and the frequency of general elections is upwardly bounded since there is normally considerable resistance to intervals shorter than two to three years between elections. Yet the magnitude of the observed relationship is noteworthy. The path model in Figure 1 illustrates that, apart from this strong direct impact, electoral salience has several indirect effects on government durability: through crisis duration, formation attempts and parliamentary basis. The indirect effect through formation attempts is weak and positive; the others are negative. But the strongest relationship is clearly the direct and positive one.

The third significant relationship in Table 7 links crisis duration and cabinet duration. The longer the preceding crisis, the longer the government tends to last. But the tradeoff is hardly very favorable, since an extra day of cabinet crisis buys little more than a day of cabinet durability. In addition to this rather modest effect, crisis duration has an indirect effect on cabinet durability, because there is a weak tendency for majority coalitions to result from more protracted crises than minority governments. The relationship between crisis duration and government duration may involve an inhibition on early dismissals of governments formed after lengthy negotiations. Such governments may tend to survive not only because the various parties have a vivid image of the costs of another round of difficult negotiations, but also because the previous round presumably cleared away the most immediate obstacles to government unity.

### Subsequent Alternation

Table 8 presents the equation with subsequent alternation as the dependent variable. Again, there are few significant relationships, and the bivariate relationship between parliamentary basis and government performance tends to hold up when the various control variables are introduced. However, in this case only the final equation produces a significant difference between majority coalitions and substantive minority governments. There is close to 9% less turnover associated with majority coalitions, a discrepancy that is somewhat smaller than in the bivariate analysis. Polarization and the concentration of the opposition are actually stronger predictors of alternation than is parliamentary basis, but none of these relationships is particularly impressive. Polarization is the strongest predictor of alternation, as high levels of polarization reduce the degree of turnover in government. This is a reasonable finding; nor should we be surprised to find that alternation increases with the concentration of the opposition. The further apart the various opposition parties are ideologically, and the more the government can split them into separate blocks, the lesser the opportunities for turnover in office. Majority size is an additional obstacle to alternation. In the full equation in Table 8, the effects of numerical status, polarization, and opposition concentration are all depressed by the inclusion of electoral salience. Figure 2 confirms these relationships. Note also that although electoral salience does not directly impinge on alternation, there is a positive indirect effect through parliamentary basis. On the other hand, electoral salience indirectly inhibits alternation through its impact on formation attempts, but this link is very tenuous.

From the point of view of the governing parties, nothing could be better than a situation that inhibits alternation in office. At least it would appear so in the short run; over time a situation without exit may become burdensome for the parties in government. High levels of polarization, fragmentation of the opposition, and large coalitions are a recipe for stability in government, but in systemic terms these factors represent an immobilist syndrome. Specifically, these conditions correspond to critical properties of the type of party system Sartori calls polarized pluralism (Sani & Sartori, 1983; Sartori, 1976, pp. 131-145). Hence, to the extent that government minoritarianism and electoral salience produce alternational party systems, these factors may in fact promote systemic stability and responsiveness.

### Electoral Success

The final regression model has electoral success as its dependent variable. Table 9 presents the results. Again, there are few strong relationships to report. Numerical status emerges as the strongest predictor of electoral success, and the introduction of various controls does little to reduce the advantage enjoyed by minority cabinets over majority coalitions. This advantage amounts to approximately 2.5% of the popular vote. Two other variables exhibit significant relationships. Electoral salience is the strongest predictor in the full equation, and it is worth keeping in mind that this variable has a considerable indirect impact through its conduciveness to minority government formation. Both directly and indirectly electoral salience improves government performance at the polls. However, this impact means reduced government losses rather than increased gains, since there is a clear tendency for all types of cabinets to suffer rather than gain at the polls.

Table 8. Multivariate Analysis of Subsequent Alternation

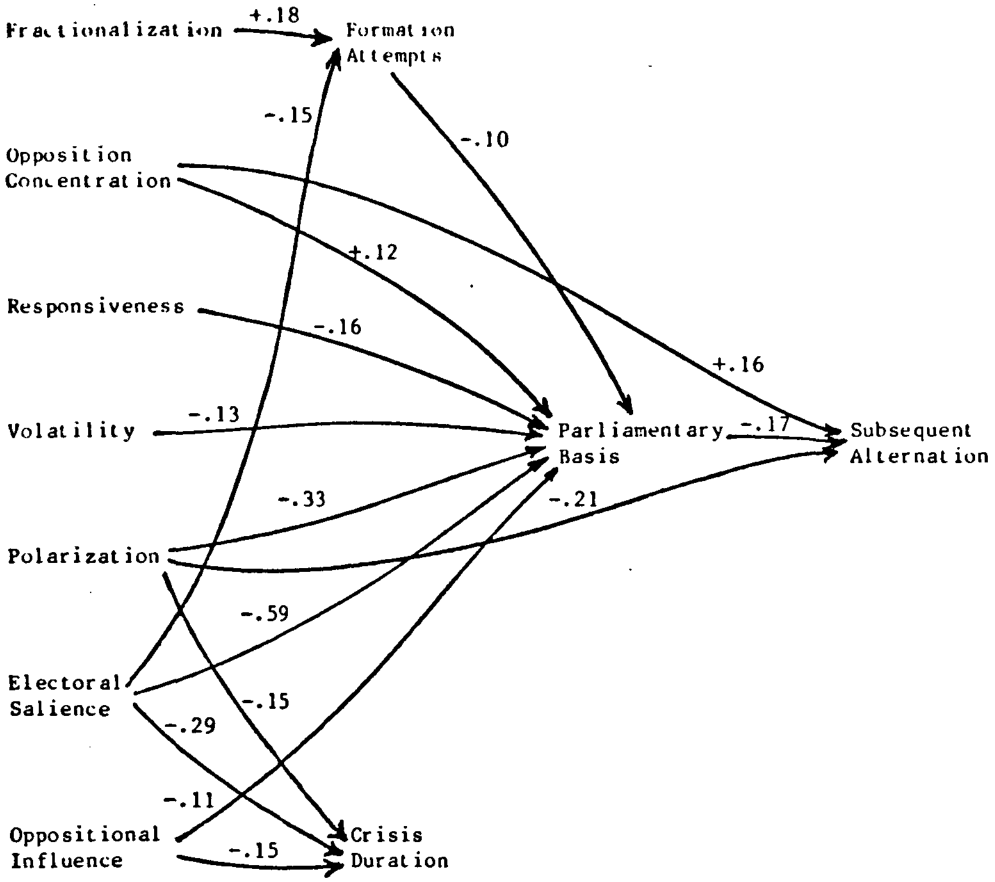
| Variable | Full Equation Coefficient | Full Equation SE | Full Equation Beta | Final Equation Coefficient | Final Equation SE | Final Equation Beta |
|---|---|---|---|---|---|---|
| Numerical status | −5.22 | 4.45 | −.09 | −8.82** | 3.76 | −.14 |
| Influence of opposition | −.65 | 2.25 | −.02 | | NS | |
| Electoral salience | 4.48 | 3.67 | .13 | | NS | |
| Volatility | 29.64 | 32.28 | .06 | | NS | |
| Responsiveness | 8.00 | 9.99 | .06 | | NS | |
| Polarization | −.26 | .25 | −.10 | −.46** | .17 | −.18 |
| Fractionalization | 12.16 | 40.52 | .03 | | NS | |
| Crisis duration | .02 | .05 | .02 | | NS | |
| Formation attempts | 1.04 | 1.41 | .05 | | NS | |
| Opposition concentration | 30.31* | 15.25 | .14 | 35.34** | 14.01 | .17 |
| Investiture | −3.73 | 4.42 | −.06 | | NS | |
| Adjusted $R^2$ | | .097 | | | .105 | |

$N = 241$

NS = Not significant at .05 level

*Significant at .05 level
**Significant at .01 level

Figure 2. Causal Path Model of Subsequent Alternation


Fractionalization
+.18
Formation Attempts
−.15
−.10
Opposition Concentration
+.12
Responsiveness
−.16
+.16
Volatility
−.13
Parliamentary Basis
−.17
Subsequent Alternation
−.33
−.21
Polarization
−.59
Electoral Salience
−.15
−.29
−.11
Oppositional Influence
−.15
Crisis Duration

Note. Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

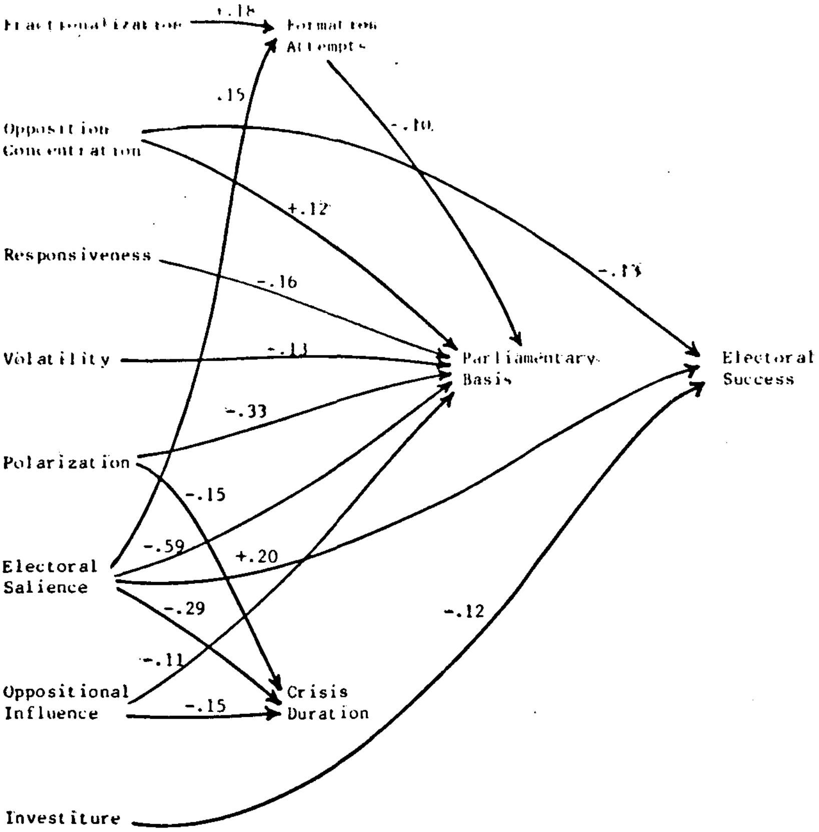
Table 9. Multivariate Analysis of Electoral Success

| Variable | Full Equation |  |  | Final Equation |  |  |
|---|---|---|---|---|---|---|
|  | Coefficient | SE | Beta | Coefficient | SE | Beta |
| Numerical status | −2.32* | 1.01 | −.17 | −2.92** | .86 | −.22 |
| Influence of opposition | .17 | .52 | .02 |  | NS |  |
| Electoral salience | 1.87* | .85 | .25 |  | NS |  |
| Volatility | −10.75 | 7.18 | −.10 | −12.53* | 6.70 | −.12 |
| Responsiveness | −1.69 | 2.41 | −.05 |  | NS |  |
| Polarization | .06 | .06 | .11 |  | NS |  |
| Fractionalization | 3.81 | 9.08 | .04 |  | NS |  |
| Crisis duration | .01 | .01 | .06 |  | NS |  |
| Formation attempts | .01 | .31 | .00 |  | NS |  |
| Opposition concentration | −4.78 | 3.43 | −.12 |  | NS |  |
| Investiture | −1.11 | .97 | −.10 |  | NS |  |
| Adjusted $R^2$ |  | .049 |  |  | .045 |  |
| $N = 241$ |  |  |  |  |  |  |

NS = Not significant at .05 level

*Significant at .05 level
**Significant at .01 level

Figure 3. Causal Path Model of Electoral Success


Fractionalization
+.18
Formation Attempts
.15
Opposition Concentration
-.10
+.12
Responsiveness
-.16
-.13
Volatility
-.13
Parliamentary Basis
Electoral Success
-.33
Polarization
-.15
Electoral Salience
-.59
+.20
-.29
-.12
-.11
Oppositional Influence
-.15
Crisis Duration
Investiture

Note. Numbers are standardized regression coefficients significant at .05 level. Minority situations with partisan outcomes only.

Electoral volatility, on the other hand, depresses the electoral fortunes of incumbent governments. This is another reasonable relationship. Because electoral shifts generally work against the incumbents, volatility should be no boon. The indirect effect of volatility through parliamentary basis, however, is to enhance government success at the polls. The effects of electoral salience and volatility do not both reach significance at the .05 level in the same equation. Electoral salience is significant only in the full equation, volatility in the final equation. However, both qualify at the .1 level in the same model. As in the case of alternation, it is largely the elimination of electoral salience that boosts the effects of numerical status and volatility in the final equation.

The causal pattern changes considerably when the continuous measure of parliamentary basis is used. Figure 3 shows that opposition concentration, the existence of investiture requirements, and electoral salience affect electoral success in this case. Government success at the polls is thus enhanced by a dispersed opposition and by a political system geared toward electoral contests. The modest impact of investiture requirements has no obvious explanation. The advantages of parliamentary smallness barely fail to reach significance with the continuous variable in the equation. Electoral fortunes apparently do not diminish in a linear relationship to the size of the government. Either numerically very large governments do not perform particularly poorly, or very undersized governments do not do especially well, or both. At least the former possibility is plausible enough. Because grand coalitions tend to form during periods of national emergency (e.g., war), they may be well insulated against electoral setbacks. The electoral superiority of minority governments, then, is probably strongest at the middling range of the numerical spectrum.

## Conclusions

In this article I have analyzed the performance of various types of parliamentary governments. Government performance is a notoriously slippery concept, and good measures are not easily found. I have chosen to relate government performance to the objectives of political parties. This rational choice approach to government performance has several advantages over the functional analysis that has characterized much of the previous literature. From a relatively simple set of assumptions about party behavior, it is possible to derive performance criteria that are neither overly broad and vague nor excessively specific and subjective. The rationalist framework also permits theoretical and empirical linkages between the study of government performance and the analysis of coalition formation.

The choice of a rationalist mode of analysis does not prevent us from addressing problems at the systemic level. Some of the measures developed here are in fact very closely related to performance dimensions in traditional functional analysis. But a systematic effort to understand the systemic implications of the observed patterns of government performance requires further assumptions and analysis. At the most general level, I would contend that solutions that are rational for self-interested political parties normally also work well for the larger political system. This is at least partly a result of conscious political engineering to produce favorable structure-induced equilibria in the party system (see Shepsle, 1979; Shepsle & Weingast, 1981).

But partisan and systemic interests will not coincide to the same extent on all performance dimensions. Intuitively the correspondence seems higher for short-term than for longer-term performance. Government instability and ineffectiveness between elections are public bads. But long-term stability, and particularly lack of alternation in office, will eventually become immobilism. Strong governmental influence performance is also more likely to be a collective good than strong office performance. The struggle for office is clearly a zero-sum game, the quest for policy influence at least arguably positive-sum.

In the empirical section of this analysis, I have used four performance measures developed within the rationalist framework to test a theory of government formation and performance. The analysis demonstrates clear performance differences between various cabinet types. Majority party governments fare considerably better than the other types in pre-electoral performance, and about average in postelectoral performance. In the more critical (for present purposes) comparison between substantive minority cabinets and majority coalitions, the picture is more mixed. Majority coalitions are superior in office performance, whereas minority governments have the advantage in influence performance. More specifically, minority governments are somewhat less durable and associated with a higher degree of subsequent alternation. On the other hand, undersized governments resign under more favorable circumstances, and they perform substantially better in subsequent elections. An overall assessment of the success of minority governments must involve a comparative weighting of these advantages and disadvantages. There is no definitive way to make this evaluation, yet it will be my contention that minority governments have acquitted themselves well for three reasons in particular.

First, there is a substantial difference between minority governments and majority coalitions in the area of electoral success. I have on theoretical grounds suggested that this is a particularly significant dimension of government performance for party leaders, and the superiority of minority governments therefore looms large in the overall picture.

Second, it is interesting to note that the comparative advantages of minority governments are found in influence, rather than office, performance. If strong influence performance is more likely to have positive systemic consequences than strong office performance, then we should look favorably upon governments that excel in the former area.

Third, minority governments are associated with higher rates of governmental turnover than majority coalitions. In fact, the strong similarity in alternation rates between substantive minority and majority party governments suggests that substantive minority governments serve as vehicles of alternation in power under conditions that do not allow for the dynamics of a perfect two-party system. Canada may serve as the best illustration of this practice. Frequent recourse to substantive minority governments has allowed the Canadians to maintain a system of alternative single-party government despite significant deviations for the two-party model. Although incumbent parties may not find high turnover rates to their liking in the short run, there is good reason to consider this a desirable systemic property. The problems of nonalternance politics have been widely lamented and well documented, as the case of Italy demonstrates (Di Palma, 1977, Sartori, 1976).

The first of these three points suggests that minority governments are rational solutions; the latter two indicate that they are also functional. The surprisingly strong performance of minority governments is probably in the final event related to the rationality of government formation. The less incentive there is to replace an undersized government, the more effective we should expect that government to be. However, because I have used performance data to demonstrate rationality, this explanation of minority government performance cannot be tested in the present analysis. I should emphasize one additional finding that has implications for our understanding of party system dynamics. Minority governments are less likely than majority coalitions to suffer electoral setback, but at the same time more vulnerable to turnover in office. This paradox may be an indication of systemic differences associated with the formation of minority government versus majority coalitions. Political systems conducive to minority government formation appear to be particularly sensitive to electoral shifts, and minority governments thereby more acountable to the electorate than majority coalitions.

Three main conclusions emerge from the empirical analysis. First, the conventional wisdom about minority government performance is generally incorrect. Secondly, the results support the theory that minority governments represent rational cabinet solutions. Finally, the performance of minority governments appears even more favorable from a systemic perspective than from the point of view of the actors in the cabinet formation game. Minority governments are sufficiently effective solutions to be chosen freely and frequently by party leaders. In the process, the responsiveness and accountability of the democratic system may actually be enhanced.

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# An Adaptive Model of Bureaucratic Politics

JONATHAN BENDOR
TERRY M. MOE
*Stanford University*

*In this article we outline a new framework for the formal analysis of bureaucratic politics. It departs from standard neoclassical approaches, notably those of Niskanen (1971) and Peltzman (1976), in several important respects. First our approach explicitly models a system of three-way interaction among bureaus, politicians, and interest groups. Second, it allows for institutional features of each type of participant. Third, it is a model of dynamic process. Fourth, participants make choices adaptively rather than optimizing. Fifth, participants are only minimally informed.*

*The result is a dynamic model of adaptive behavior, very much in the spirit of Simon's (1947) behavioral tradition, that offers a new perspective on political control, bureaucratic power, and the "intelligence of democracy."*

For decades, students of public administration have stressed that we can only understand bureaucratic behavior by looking beyond the boundaries of administrative organization (Long, 1949; Simon, Smithburg, & Thompson, 1950). Agencies operate in continual exchange with an institutionally structured environment of politicians and interest groups, and it is insight into this system of interactions that holds the key to explanation. Popular theories of bureaucratic politics, ranging in emphasis from iron triangles (Cater, 1964; Freeman, 1955; Truman, 1951) to regulatory capture (Bernstein, 1955; Huntington, 1952; Stigler, 1971) to interest group liberalism (Lowi, 1969), are clearly in agreement on this most general of points, and empirical evidence suggests that it is surely a valid one. This consensus sets bounds on what is considered controversial within the field. We argue now about how much discretion bureaus have, the relative influence of politicians and interest groups, and how these properties vary across different agencies. We do not argue about whether politicians and interest groups are central to an understanding of bureaucratic politics.

Formal models have made some useful contributions to the area, but they have uniformly shied away from modelling the kind of three-way interaction routinely described in the empirical literature. Perhaps the two most highly regarded efforts to model bureaucratic politics are Peltzman's (1976) theory of regulatory behavior and Niskanen's (1971) theory of budgets and bureaucratic supply. Consider, very briefly, their basic features.

Peltzman builds upon the earlier work of Stigler (1971) and Posner (1974) in an attempt to place regulatory capture within a more general theoretical context. He formalizes the problem by positing a regulator who makes policy decisions about transferring wealth between members of two interest groups (in effect, business and consumers). The regulator is a bureaucrat-politician who, based on perfect information about the groups, chooses among administrative options in an effort to maximize votes. Interest group members then decide to vote for or against him based on his wealth-transfer decisions and on their abilities to organize for political action. Analysis of these supply and demand factors then leads to an equilibrium solution that provides insight into the conditions under which the regulator may be captured by one of the groups.

Niskanen's work is a pioneering effort to explain budgetary outcomes—and to show why government is "too big"—with reference to special properties of the relationship between bureaucrats and legislators. He argues that bureaucrats value larger budgets (and perhaps "slack"—see Niskanen, 1975), and that they have distinct advantages over legislators in the bargaining game that determines budgetary outcomes: they have a near monopoly over information about the true costs of production, they are perfectly informed about legislative demand for their services, they are able to control the legislature's agenda by submitting

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take-it-or-leave-it budgetary proposals, and they are often aided by high-demand legislative committees. By putting these weapons to use, bureaucrats are able to win the bargaining game, securing larger budgets and (if they like) operating inefficiently.

Peltzman and Niskanen thus contribute interesting new ways of thinking about two of the most important issues of bureaucratic politics: which constituencies win and which lose, and how the level and efficiency of bureaucratic provision compare to the social optimum. Peltzman addresses the first, offering an innovative perspective on the relationship between clientele characteristics and bureaucratic resource allocation. Niskanen addresses the second, shedding new light on the relation between bureaucratic power and governmental outcomes. Yet their analyses are also based on five distinctive simplifications that, in imposing analytical structure on bureaucratic politics, condition the questions they raise and the conclusions they derive. In considering whether these assumptions seem reasonable, we are immediately struck by the most obvious of the five. 1) Both theories clearly ignore fundamental dimensions of political interaction. Peltzman combines politicians and bureaucrats into one actor, implicitly assuming that the dynamics of political control, such as oversight, budgeting, divergence of bureaucratic and political goals, and competition for control among politicians, make no significant difference for bureaucratic behavior. Niskanen gives no systematic attention to clientele groups, assuming their demands are represented by the legislature; and he models the legislature as a passive participant that submits to bureaucratic dominance without putting its own substantial resources to strategic use (see Miller & Moe, 1983). In neither is serious attention given to the distinct, interdependent roles of bureaucrats, politicians, and interest groups.

They also share several other properties which, though less objectionable, are major simplifications that structure their analyses. 2) Both make no real attempt to disaggregate institutional decision makers. Bureaus, legislatures, and interest groups are essentially treated as individual decision units. 3) Both are exercises in comparative statics. They are not dynamic models of interaction, and they tell us nothing about the political process that gives rise to their equilibrium results. 4) Both assume that all decision makers optimize, and thus that they choose on the basis of highly complex calculations beyond the capacity of most people. 5) Both include assumptions of perfect information (although Niskanen does so asymmetrically—the bureau is perfectly informed, the legislature is not).

All of these features are in fact quite common in neoclassical economic models, and they are surely understandable responses to the great complexity of bureaucratic politics. It is one thing for political scientists to point to multiparty interaction, dynamic processes, institutional context, uncertainty, limitations on cognition, and the like, and quite another to construct models that somehow incorporate these properties. In all modelling efforts, simplification is absolutely essential. The problem is always to figure out which aspects can safely be assumed away and which cannot—recognizing that there are different ways to simplify, and that even the most elegant, deductively powerful models run the danger of being very misleading if founded on simplifications of the wrong kinds.

In this article we outline a model—really, a general framework accommodating a whole family of models—that allows us, via computer simulation techniques, to pursue the same basic issues of constituency influence and governmental size addressed by Peltzman and Niskanen, yet also reflects a very different and more general approach to the formal analysis of bureaucratic politics. It departs from their models along each of the five simplifying dimensions noted above. 1) Most important, it is a model in which the central outcomes of interest—governmental outputs, budgets, and bureaucratic efficiency—are in every sense jointly determined by the interdependent decisions of bureaucratic, legislative, and interest group participants. Attention focuses on the entire system of political relationships and on the integral roles these participants play as part of that larger system. 2) It allows us, if we like, to incorporate institutional features of each type of participant, for example, by recognizing decision processes internal to bureaus or legislatures. In this article we take one of many possible steps in this direction by introducing a majority-rule legislature. 3) The model is dynamic, allowing us to explore the process of interaction and adjustment as it unfolds over time. 4) Decision makers are assumed to adapt in simple ways to their environments, moving in directions that appear to promise them greater utility. They do not optimize, nor do they carry out complex calculations. 5) Participants are only minimally informed about their environments and about each other, basing their adaptive decisions on feedback about the success of their prior decisions.

These elements have a certain natural compatibility. A model of three-way interaction is intrinsically complex, and the assumption of optimization would only magnify the problem by forcing inquiry into an array of complicated game-theoretic considerations. Because formally incorporating a third actor jeopardizes the mathematical tractability of bureaucratic models in the neoclassical tradition, it is no accident that they address only dyadic interaction.[1] Assumptions of adaptive behavior, on the other hand, are not only far more reasonable empirically, but they are also particularly well suited to the task of modelling dynamic, interactive decision making under uncertainty. From a purely technical standpoint, it is a relatively easy matter to model individual decision makers in terms of utility functions and adaptive rules, impose a structure (reflecting, e.g., institutional context) on their interactions, and then allow a computer to map out the corresponding implications for individual choices and collective outcomes as they occur over time. Large numbers of interacting decision makers and varying institutional contexts can be handled without any real difficulty, given the computer's admirable skill at calculation. This technical feasibility does not, of course, imply that any given model will provide an adequate or insightful explanation of bureaucratic politics. But it is an important advantage which, in combination with the greater realism of its assumptions, sets this approach apart from the conventional neoclassical approach and makes it particularly worthy of investigation.

Thus, our interest in constructing a model that incorporates a basic fact of political life—interaction among bureaucrats, politicians, and interest groups—has led us to follow a strategy that departs from the norm. What we are proposing, in fact, is a dynamic model of adaptive behavior that is very much in the spirit of the behavioral tradition associated with Simon (1947), March and Simon (1958), Cyert and March (1963), Crecine (1969), Cohen, March, and Olsen (1972), Axelrod (1976), Padgett (1980), Cohen (1981, 1984), and Cohen and Axelrod (1984). Works in this tradition, grounded in the limitations on human decision makers, have long pointed to adaptation and dynamic process as central to an understanding of organizational behavior, and they have pioneered in the development of computer-assisted models of organizational behavior. Because our model is in some sense an application of this theoretical perspective to the study of bureaucratic politics, its success (or lack of it) says something not only about the nature of bureaucratic politics, but also about the value of putting Simon's behavioral tradition to new use.

## The Structure of the Model

Our general framework can be applied in a variety of ways. Here we focus on bureaucratic behavior in a classic type of political environment—one characterized by interest group conflict—and we develop a simple model to explain bureaucratic output, budgets, and efficiency. The model's central components are an agency interested in some combination of bigger budgets, more slack, achieving policy goals, and avoiding oversight;[2] a legislature made up of 101 elected politicians, each of whom is interested only in getting re-elected;[3] and two interest groups, one that benefits from the agency's program and one that is hurt.[4] None of this is anchored in particular agencies or policy areas. For purposes of illustration, however, we will develop the analysis below with reference to an agency engaged in consumer protection regulation. On this interpretation, which is obviously just one of many possibilities, the agency's output is taken to be the level of regulatory enforcement, and its enforcement activities are assumed to be supported by consumers and opposed by business.

In broad outline, the model is designed to reflect the circular flow of influence characteristic of representative government. Citizens pressure legislators through elections, legislators influence the bureau through budgets and oversight, the bureau affects citizens through the costs and benefits generated by regulatory enforcement—and the circle is closed when citizens link their electoral support to legislators' positions on agency-relevant issues. This iterative process works itself out in the following way.[5] (See Figure 1.)

Every period, the legislature transmits to the bureau a new budget that may differ by as much as 10% from its budget in the prior period. Each

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[1] For an interesting exception relying on general equilibrium analysis, see Fiorina and Noll (1978).

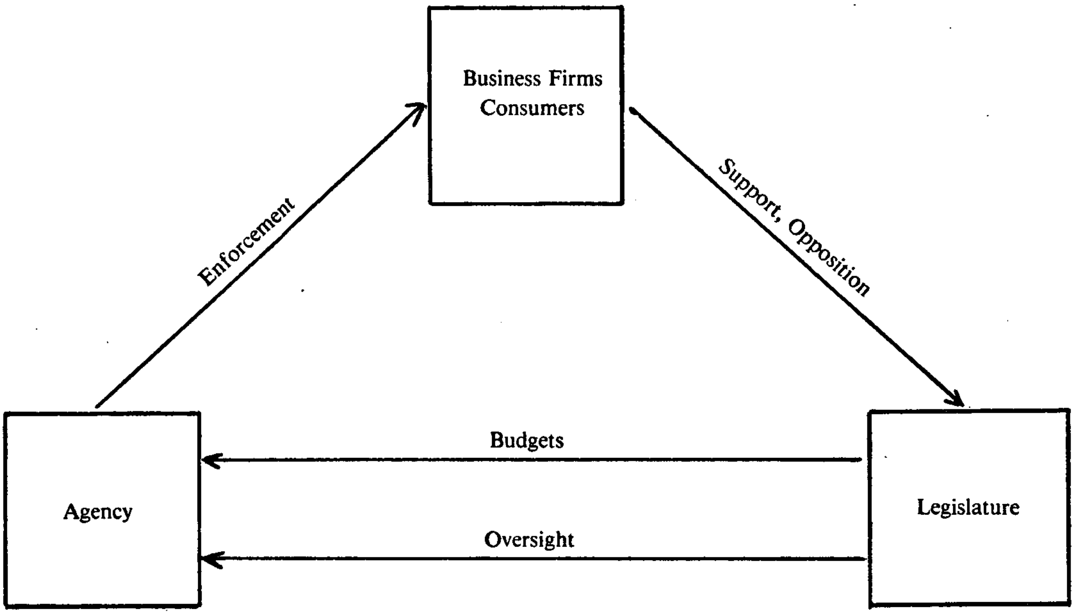
[2] Although there is no agreement in the literature on exactly what motivates bureaucrats, these elements are prominently mentioned. Note that our model allows the agency to value just one of these, or any weighted combination, giving us a basis for exploring the implications of alternative bureaucratic goals.

[3] On the importance of the re-election incentive, see Mayhew (1974) and Fiorina (1974). We chose 101 for the size of the legislature because it is large enough to prevent the institutional dynamics from being obscured by the idiosyncratic behavior of one or a few legislators, and as an odd number, it allows for a clear majority winner. Had we chosen a larger (odd) number, say 501, the model's results would be identical in all essential respects to those presented here.

[4] These groups need not be monolithic organizations, and may even be sets of unorganized citizens. We think of them, abstractly, as sectors that generate votes, money, and other resources in response to political outcomes. Their comparative effectiveness in doing so can be varied by parameter settings of the model.

[5] Readers interested in the finer details may write the authors for a copy of the computer program.

Figure 1. The Flow of Influence



legislator must take a position somewhere on a scale between -10 and +10 percent.[6] In deciding how to vote, the individual legislator compares his past votes with past changes in the mix of support and opposition he has received from the two interest groups, that is, changes in his electoral security. If he changed his position in a pro-agency direction last period—say, by moving from +2.5 to +3.1 on the budget scale—and his electoral security subsequently increased, then he will want to take another step in this direction, with the size of the step determined by the size of his utility gain. Similarly, had this earlier move been sanctioned by a drop in electoral security, he would now reverse course and support a smaller, perhaps even a negative, budget increase. The same kind of adaptive logic applies for other possible permutations. Once each legislator has adopted a new position, the entire legislature votes and the median position determines the new budget that is transmitted to the agency.

The legislature also engages in oversight, although it is oversight of a very limited sort. It is activated in the current period only if the legislature increased the agency's budget in the prior period and subsequently discovers, via feedback from the interest groups, that the agency produced less enforcement with this larger amount of money.[7] This obvious inefficiency results in "hassling" from the proconsumer legislators (those occupying positions greater than 0 on the budget scale), and the intensity of hassling varies with the political weight of this consumerist contingent (i.e., with the number of legislators to the right of 0, and how far to the right they are on average). If oversight is not triggered in the current period, then the level of hassling decays exponentially.

The legislature's work ends once the new budget and hassling levels are decided. These new quantities are now inputs to the agency that alter its utility, telling it whether the environment is approving or disapproving of its past behavior. As noted, its utility function contains one or more of four potential components—budget, slack (the amount of the budget not devoted to enforcement), hassling, and policy (in the form of an enforcement level the agency prefers to all others, its ideal point)—and therefore the legislature's impact on its utility will depend in part on precisely what combination of these the agency happens to value. When relevant, budget and slack contribute positively to utility, whereas both hassling and departure from the agency's ideal point contribute negatively. Once the agency has assessed its utility change, it follows an adaptive strategy in setting a new value for its choice variable, efficiency. Thus, if it increased its efficiency last period and was rewarded with an increase in utility, it will increase efficiency again in the current period. And so on for the other logical possibilities.

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[6] Initial positions are assigned by random draw from a normal population with mean 0 and standard deviation 4. Hence, the first change in the budget equals the median vote of this random sample. The random character of the first budgetary change does not affect the model's basic results.

[7] This oversight strategy, similar in spirit to the fire-alarm monitoring analyzed by McCubbins and Schwartz (1984) and Weingast (1983), economizes on the information-gathering resources of the legislature.

Efficiency is defined as the fraction of the budget spent on enforcement, and therefore ranges between zero and one. Enforcement is generated as a linear function of the actual amount of money spent on it—that is,

$$\text{enforcement} = c \cdot \text{efficiency} \cdot \text{budget}, \qquad (1)$$

where $c$ is a constant representing the units of enforcement purchased by a unit of spending.[8]

Having chosen a new level of efficiency, the agency generates a new level of enforcement which in turn yields new levels of costs for business and benefits for consumers. The business costs imposed by regulation increase at an increasing rate (increasing marginal costs), while benefits to consumers increase at a decreasing rate (decreasing marginal benefits).[9] Specifically,

$$\text{benefits} = k1 \cdot \text{enforcement} - k2 \cdot \text{enforcement}^2 \qquad (2)$$

$$\text{costs} = k3 \cdot \text{enforcement}^2 \qquad (3)$$

where $k1$, $k2$, and $k3$ are positive constants. In our initial version of the model, consumers evaluate the benefits of regulation without taking into account the taxes needed to finance the agency. In the final version, consumers base their evaluation of regulation on net benefits: the value of enforcement minus its tax costs.[10] Under both versions the social optimum is the point at which enforcement's marginal costs and marginal benefits are equal, with budgetary costs incorporated into the calculation.

For both business and consumers, the political stakes rise as enforcement rises, and both therefore devote more resources (campaign contributions, votes, etc.) to the electoral process in support of politicians who promote their interests. Consumers increase their contributions in proportion to the benefits they derive from enforcement; firms increase their contributions in proportion to the costs imposed on them:

$$\text{consumer's political resources} = k4 + k5 \cdot \text{benefits} \qquad (4)$$

$$\text{firms' political resources} = k6 + k7 \cdot \text{costs}, \qquad (5)$$

where $k4$ through $k7$ are positive constants. (See Figure 2.) Thus, the more an interest group is helped by a program, the more it is willing to contribute to politicians supportive of the agency. Similarly, the more a group is hurt by a program, the more it is willing to contribute to politicians who oppose the agency. Empirically, however, the existing system of interest organizations is unbalanced in favor of business, and this imbalance is enhanced by free-rider problems that disproportionately plague the emergence of new consumer groups (Olson, 1965; Stigler, 1971). There is accordingly a closer correspondence in the model between additional costs imposed on business and its political contributions than is true of the analogous benefit-to-contributions translation for con-

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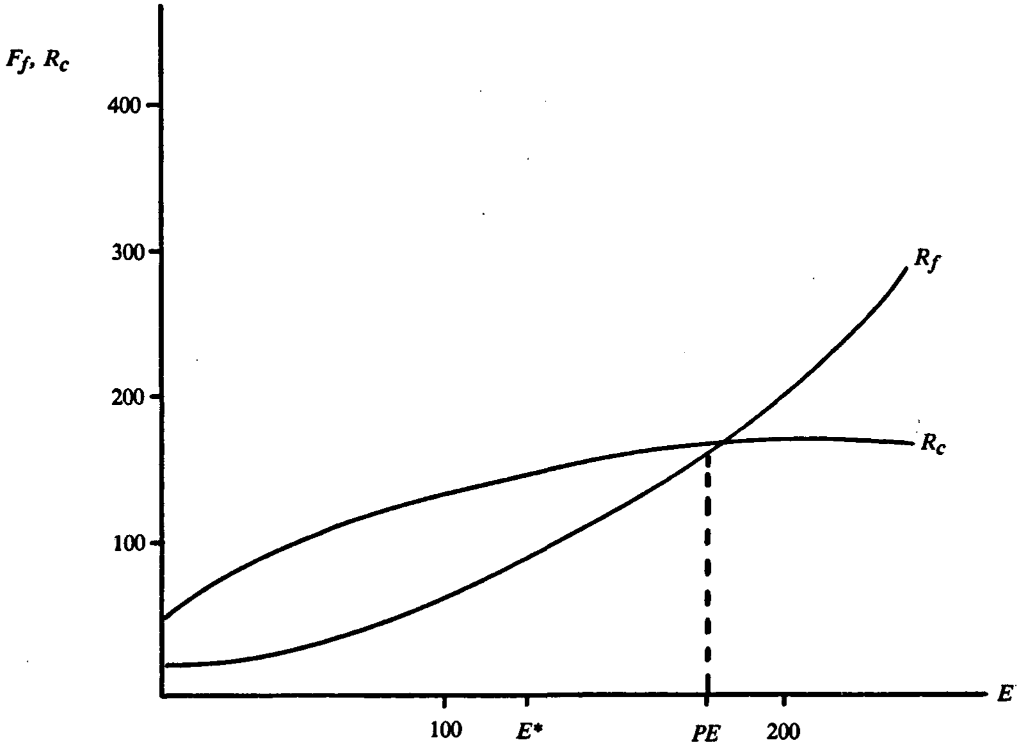
[8] One reasonable interpretation of enforcement "units" is person-hours devoted to enforcement. Because there is no unique scale for such units, $c$ can be any positive constant; for convenience, we assume throughout that $c = 1$. Similarly, there is no unique scale for measuring budgets. Although the budgets of real agencies can range from the millions to the billions, for simplicity—and because the scale does not affect the model's basic results—we have set the initial budget equal to 100. This, together with our assumptions about efficiency and the constant $c$, determine the scale for enforcement.

[9] This is a standard assumption and is certainly a reasonable place to start. Yet costs and benefits may take on far more complex patterns empirically, and they may be interrelated in various ways—as, for example, when regulatory costs bring about business failures and subsequent loss of jobs for consumers. Our curves are not inconsistent with many of these scenarios (e.g., diminishing marginal benefits for consumers captures the notion that the rewards of enforcement are diluted by its negative by-products), but new work along these lines, empirical as well as analytical, is clearly needed if our theories are ultimately to be well designed.

[10] Because the budgets of regulatory agencies are only a tiny fraction of the federal budget, it is a reasonable first approximation to assume that an agency's beneficiaries are more aware of benefits than tax costs. Moreover, we gain a clearer understanding of the distinctive contributions of the model's various components by dealing first with the simple nontax world and introducing taxes later.

**Figure 2. The Pluralist Equilibrium in the Taxless Model**



*Cost and Benefit Functions (not graphed)*

Costs $= .023E^2$
Benefits $= 20E - .04E^2$

*Resource Functions*

Firms $R_f = 10 + .20\,(.023E^2)$
$= 10 + .20$ (costs)

Cons. $R_c = 50 + .05\,(20E - .04E^2)$
$= 50 + .05$ (benefits)

$E$ = Enforcement
$PE$ = Pluralist Equilibrium
$E^*$ = Social Optimum
$R_f$ = Political Resources of Firms
$R_c$ = Political Resources of Consumers

sumers. (In equations (4) and (5), constant $k5$ is smaller than constant $k7$.)[11]

Once business and consumer groups decide upon resource levels, they expend them for or against individual legislators in accordance with the latter's positions on agency budgets. Legislators favoring budgetary increases are judged as pro-agency and therefore as proconsumer; they get resource support from consumers and resource opposition from business in amounts that are a positive function of the size of the budget increase they favor.[12] The same logic applies for legislators favoring reduction in the agency budget.

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[11] Note that our interpretation of organizational advantage is that firms are better able to respond to changes in enforcement than are consumers. This in itself says nothing about the absolute level of resources when enforcement is zero. We assume, for present purposes, that when there is no enforcement, consumers have more political resources than firms do. This makes sense for the following reasons: consumers are numerous, they vote, voting requires little organization, and when there is no regulation, the issue is likely to be salient to voters. Moreover, the opposite assumption is easily accommodated by our model, as we suggest in note 24 below.

[12] In versions of the model where consumers take taxes into account, consumers will support legislators voting for smaller budgets if the taxes required to finance the

Legislators now find that owing to their earlier adaptive shifts in position on the budgetary scale, they face altered patterns of electoral support and opposition—that is, changes in their utility—anchored in the adjusted evaluations of business and consumer groups. This new feedback prompts them to adapt by choosing a new budgetary position for the coming period, following the decision rule outlined above. This completes the circle and sends the process into another iteration.

Before summarizing the results, we would like to pause briefly to underscore a few basic aspects of this model. First, all of its decision makers are self-interested: they seek to realize their own goals, whatever they might be, and they are not directly concerned with the well-being of other participants.[13] Thus, if the model generates societally attractive outcomes, or if one participant makes decisions beneficial to one or more of the others, these will rarely be intended consequences, but rather the by-products of self-interested behavior. In this respect, we adhere to a central theme of rational choice models of regulation and bureaucracy, and we adhere as well to a classic line of pluralist theory.

Second, our participants are endowed with very little knowledge. Legislators do not know the marginal electoral value of another dollar added to the regulatory agency's budget. Bureaucrats do not know the marginal gain of one more unit of enforcement. What these decision makers do know is rather modest. Legislators know such things as how they voted on the previous appropriations bill and how full their campaign warchests are. The regulatory agency knows only what percentage of the budget it has devoted to enforcement and how well off it is; it knows nothing directly of the effects of regulation, and it must choose a level of efficiency in ignorance of the true consequences of its actions. In short, as incrementalist analyses of the political process have long contended, these participants adapt as best they can based on what little they know: They muddle through (Braybrooke & Lindblom, 1963; Lindblom, 1959).

Third, our participants are simple adaptive decision-makers. They learn about their environments only to the extent that they link new information about changes in behavior with new information about changes in utility, and they adapt by following a trial-and-error procedure that prompts them to repeat rewarded actions and to avoid sanctioned ones. They do not generalize or otherwise develop belief structures about their environments, nor do they try to guess the future. The specific model we use here, then, assumes perhaps the simplest possible adaptive scheme—a reasonable place to start, given that we are moving into uncharted territory. More complex adaptive strategies can always be introduced once simple models of this sort are better understood.[14]

Fourth, the distribution of influence is typically pluralistic: no one is in charge. The structure of modern democratic government virtually guarantees that citizens, politicians, and bureaucrats will interact through a network of relationships in which no single participant can truly dominate or be entirely autonomous, and in which each must adapt to the decisions of others.

Finally, the model emphasizes the role of indirect influence, of the importance—more, the necessity—in American politics of getting what you want via other decision makers. The bureau likes bigger budgets, but cannot directly influence the legislature. Its behavior directly affects the interest groups, but these groups cannot influence the agency directly; they must try to induce the legislators to move the bureaucracy in the desired direction. The legislators' electoral fortunes are directly affected by the groups, but in order to better their chances the legislators must work through the agency. These indirect paths of influ-

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bureau outweigh the benefits of regulation. Typically, however, consumers help legislators voting for larger budgets and oppose those voting for reductions.

[13] This is not invariably true. In some runs the agency has policy goals that do not derive from preferences for budget or slack. These policy goals could derive from more general, nonegoistic ideologies. But the general sense of the model is of self-interested behavior.

[14] The adaptive strategy our actors follow, a simple trial-and-error procedure operations researchers call "hillclimbing," is a weak strategy: it gets trapped on local maxima, makes inferential errors, and is often quite slow. And its implied cognitive processes underestimate the sophistication of real decision makers. But positing trial-and-error yields a significant modelling benefit: although a weak strategy, it is highly general (Rich, 1983). Unlike most optimization techniques, hillclimbing can be used in an extraordinarily wide variety of task environments—singlepeaked or multipeaked, linear or nonlinear, deterministic or stochastic—and the modeller can avoid specifying detailed beliefs and heuristics of the decision makers. In contrast, when psychologists model a problem-solver facing a particular task, they find that the heuristics are extremely context-specific. Chess heuristics do not closely resemble theorem-proving heuristics (Newell & Simon, 1972). Since political scientists are more interested in systemic behavior than in individual behavior, we are willing to trade some accuracy at the microlevel for generality at the macrolevel, and in the class of adaptive strategies, hillclimbing is one of the most general.

ence and the uncertainties they create are the hallmarks of democratic government.

## Simulation Results

A computer model of this sort necessarily has many parameter settings, and this can obscure its central tendencies. To highlight major patterns, we will first present simple versions and then add complications one at a time.

### Version 1: Budget-Maximizing Agency

Clearly, an agency with only one goal is simpler to analyze than an agency with several. Consistent with most formal theories of bureaucracy, our baseline version is a regulatory agency that cares only about budgets. The outcomes generated by this system—the equilibrium levels of enforcement, efficiency, and budget—are presented in Table 1. We suspect they surprise most readers. Formal models of bureaus, from Niskanen on, have typically concluded that budget maximization leads either to gross inefficiency or gross oversupply of output. Indeed, this is approaching the status of conventional wisdom. But our model suggests quite a different effect of budget maximization: that it can make an agency responsive to the power configuration in its environment.

The system's logic of adjustment is easy to discern. The agency quickly discovers that increases in its efficiency are rewarded by the legislature—whose members, unbeknown to the agency, are disproportionately rewarded by consumer groups for supporting budget increases; this occurs because, at the initial enforcement level of 80 (an arbitrary starting point), consumer groups outcontribute business groups, and many legislators are motivated to shift to the right on the budget scale.[15] The agency adapts by jacking up its efficiency. The combination of higher efficiency and bigger budgets provided by the legislature rapidly increases enforcement. This increase, however, eventually mobilizes business, and the consumers' early success proves to be self-limiting. Once business outspends consumers (which, given equations (4) and (5), occurs after enforcement exceeds 184), legislators tend to be rewarded for cutting rather than increasing the agency's budget. The legislature therefore reverses course—yet it will not go far below 184, for then consumers begin to outspend business again, and a legislator who continues to vote against the agency will tend to see his opponent's war chest rise.

These oscillatory adjustments damp out over time as the system settles down to its equilibrium enforcement level of 184: the level at which the

Table 1. Simulation Results

| Version | 1 | 2 | 3 | 4 | 5 | 6A | 6B |
|---|---|---|---|---|---|---|---|
| Parameters | | | | | | | |
| Budgets | Yes | Yes | Yes | Yes | Yes | No | No |
| Slack | No | Yes | No | No | Yes | No | No |
| Policy | No | No | No | No | No | Bus | Con |
| Oversight | No | No | No | No | Yes | No | No |
| Adaptation | Fast | Fast | Fast | Slow | Fast | Fast | Fast |
| First Move | Incr | Incr | Decr | Decr | Incr | Incr | Incr |
| Nontax model | | | | | | | |
| Enforcement | 184 | Deg | Deg | 184 | 184 | CR | 184 |
| Budget | 249 | Deg | Deg | 360 | 371 | CR | 196 |
| Efficiency | .74 | Deg | Deg | .51 | .50 | CR | .94 |
| Tax model | | | | | | | |
| Enforcement | 178 | 87 | 56 | 171 | 170 | 101 | 174 |
| Budget | 210 | 1509 | 1485 | 321 | 382 | 1473 | 197 |
| Efficiency | .85 | .06 | .04 | .53 | .45 | .07 | .88 |

*Key.* Deg, Degenerate (enforcement and efficiency $\cong 0$)
CR, Compromise
Con, Proconsumer
Bus, Probusiness
Incr, Increase
Decr, Decrease

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[15] We experimented with different initial enforcement levels. Equilibrium outcomes remained the same.

political resources of business and consumer groups are equal (as described by the intersection of their resource functions—see Figure 2). We call this the pluralist equilibrium, because it obviously formalizes the old pluralist idea of a balance of group forces. As the system approaches this equilibrium level, the other key variables—efficiency, the budget—hit steady-state values consistent with it, and legislators become tightly distributed around the median position of 0 on the budget scale.

In a pluralist political environment, then, the budget-maximizing agency of Version 1 cannot impose output levels that exceed the amount determined by the environment's power balance. Here, unlike in the usual formal models of bureaus, any oversupply of output is due not to the agency's lust for revenues, but to a power mismatch in the bureau's environment.[16] Because legislators are responding to electoral rewards and sanctions from interest groups rather than to the blandishments of the bureau, the legislature simply refuses to provide the agency with appropriations that, combined with its high efficiency, would yield so much enforcement that the electoral chances of incumbents would be dimmed. The agency would surely like to obtain much larger budgets—but legislators have no incentive to go along, and they hold the purse strings. Empirically, this result makes perfect sense.

In part, the intuitive appeal of the excessive output hypothesis derives from a specific class of policy sectors, where the high demand interest group is well organized. This includes porkbarrel projects (Shepsle & Weingast, 1981) and, more generally, policies that create compact winners and diffuse losers. But, as Wilson (1980) has argued, many policy sectors lack this property, and in these the excessive output hypothesis is less plausible. Many regulatory agencies either have compact losers as well as compact gainers (the NLRB, the ICC) or, still worse from the standpoint of the excessive output hypothesis, diffuse gainers and compact losers (the EPA, the Consumer Product Safety Commission). Our model incorporates Wilson's insight that the relative compactness of interest sectors influences bureaucratic outcomes. It does so by means of two parameters representing the fraction of the benefit or loss deriving from enforcement that is subsequently translated into political resources. The more compact the group, the higher the fraction. Because empirically business is more compact than consumers, the business parameter exceeds the consumer parameter in all versions of our model, and the pluralist equilibrium is therefore farther to the left—that is, the two resource curves intersect at a smaller level of enforcement —than would be the case if the groups were equally able to translate costs and benefits into political resources. Marginal changes in parameters from these settings have predictable effects. As consumers become more compact, the pluralist equilibrium shifts to the right, and as business becomes more compact, the pluralist equilibrium shifts to the left. Because outcomes are determined by the pluralist equilibrium in this version of our model, changes in relative compactness produce—through a chain of interactive adjustments—changes in regulatory performance, which formalizes Wilson's assertion that bureaucratic outcomes reflect group compactness and diffuseness.

## Version 2: The Effect of Slack

Presumably bureaus prefer bigger budgets because of what they can buy, such as greater prestige, higher salaries, and more output or perquisites. Version 1 left those higher-order goals unspecified. We now specify one such goal: slack, defined here as appropriations devoted to any use other than output. The agency in Version 2 seeks slack in addition to larger budgets. Table 1 reveals the effect on enforcement, efficiency, and the budget. They differ dramatically from Version 1's pluralist equilibrium. In Version 2, efficiency plunges to zero. Ironically, though *all* the legislators quickly start voting in accord with consumers' desires, the consumers do not benefit at all, because enforcement is fast approaching zero despite the constantly increasing budget. This is puzzling: everyone is happy except consumers, even though they outspend firms in *every* period. Unlike Version 1, this result does not accord with intuitions based on simple power reasoning. What is going on?

The explanation is simple. A slack-seeking bureau quickly discovers that it can improve its well-being by lowering its efficiency. Because it decreases productivity faster than the legislature increases its budget, output falls. As enforcement falls, consumers continue to outcontribute firms. But though the legislature is responding to the wishes of consumers, it has only one instrument, the budget, and budgets are singularly ineffective instruments in such circumstances. A proconsumer legislature is caught in a bind. On one hand, because the more powerful interest group desires more output, it makes some sense to give the agency more money. In this line of reasoning, budgets are resources needed to produce enforcement. But if the agency is a budget-seeker, both intrinsically *and* because it has multiple uses for appropriations, then revenues are also incentives. Thus increasing the budget not only gives the bureau more resources to do its job, but also rewards it for past behavior. Here the reward is inappropriate, since the agency's inefficiency caused the problem in the first place. But if budgets are the only instrument, what else can the legislature do? It does not make sense, at least not in the short run, to deal with a problem of diminished output by denying the agency the resources necessary to produce more output. Thus the bind, produced by a combination of the bureau's monopoly position and the legislature's impoverished repertoire, generates a pathological cycle, a degenerate case of huge budgets and negligible output.

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[16] This difference between our model's results and Niskanen's reflects different assumptions about the distribution of resources rather than differences in decision-makers' goals. In his model, the deck is stacked in favor of the bureau: it has crucial informational advantages and in effect imposes outcomes on the legislature (Miller & Moe, 1983). In our model, because key resources are not monopolized by the bureau, outcomes are jointly determined. In a system with a pluralistic distribution of power, budget-maximization by a bureau does not necessarily lead to superoptimal budgets, just as in a competitive market, profit-maximization by a firm does not necessarily lead to superoptimal profits. (Indeed, in a perfectly competitive market, firms earn zero profits.) Such systems often do not exhibit simple links between individual motives and collective outcomes.

The dilemma the legislature faces in trying to use the budget as both a production mechanism and an incentive mechanism has been recognized in the empirical literature (Quirk, 1981), and it emerges quite naturally from our model. By contrast, the conventional notion associated with Migué and Bélanger (1974) and Niskanen (1975) is that slack seeking leads to smaller, more nearly optimal budgets and outputs. In their models, a preference for slack prompts the agency to maximize the difference between the budget and production costs rather than simply the size of the budget itself, and this leads it to prefer (and receive, given its alleged power) a budget-output combination that is closer to the social optimum than the combination preferred by a budget-maximizing agency. In our model, slack seeking makes the situation unambiguously worse. Budgets explode rather than shrink. Outputs (enforcement) do shrink, but they go to zero as the agency chooses to become totally inefficient. And the agency is consistently rewarded for this behavior by a proconsumer legislature that throws money at it in order to gain the electoral support of consumer groups. Other things being equal, society is far better off with an agency solely concerned with maximizing its budget.

### Version 3: Superstitious Learning

Noting that the agency in Version 2 garnered much bigger budgets than that in Version 1 did, the reader may now doubt our explanation of Version 1's behavior. If reducing productivity can procure such enormous budgets, why did the budget maximizer of Version 1 increase its efficiency? Is that adaptive behavior?

Adaptive, yes; optimal, no. The fundamental attribute of an adaptive decision-maker is pragmatism: if an action worked once, try it again. Subtle causal inferences about *why* an action paid off are avoided. This strategy opens the door to inferential errors. An action may have worked because of characteristics of the environment rather than of the decision. This is so in Version 1. Because initial enforcement is less than the pluralist equilibrium, the legislature will vote for larger budgets *regardless* of how the agency behaves. This is what learning theorists call a benign environment: anything the agency tries is rewarded. The combination of a benign environment and adaptive decision making renders the bureau's first move important, for it is the first move that, given the inevitable reward, leads the agency down one path (greater efficiency) or the other (less efficiency).[17]

The explanation for Version 1's behavior is now apparent. The agency's first move is a random choice: lacking experience to guide it, the bureau is equally willing to try increasing or decreasing efficiency. Version 1 is unlucky and increases efficiency. It is rewarded for doing so. It repeats the choice, and again is rewarded. Convinced it has discovered the road to bureaucratic paradise, it steadily boosts its productivity. The agency is the victim of *superstitious learning*: misunderstanding the causal structure of its environment, it has erroneously attributed its success to its own conduct.[18]

Confirmation of superstitious learning is provided by Version 3. Luckier than Version 1, its first move is to decrease efficiency. It too is rewarded by a larger budget. Equally convinced of its brilliance, it continues to reduce its productivity. The consequences are virtually identical to Version 2's explosive results: huge budgets, complete inefficiency, negligible output (Table 1). The control pathology of Version 2 is repeated. The proconsumer legislature teaches the bureau that inefficiency pays.

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[17] Ours is a model of deterministic adaptation: if increasing efficiency is followed by more utility, then the agency will increase efficiency in the next period with certainty. In a model of probabilistic adaptation, success would increase the odds that the agency would become more efficient. Probabilistic adaptation is empirically more realistic, and also less likely to result in superstitious learning because, even after an initial success, a decision maker has a chance of trying another alternative.

[18] It is well known that adaptive behavior in benign environments produces superstitious learning (Lave & March, 1975).

Therefore, the new conventional wisdom is not entirely wrong. If the legislature has only one instrument (the budget) and constituents ignore taxes, an adaptive budget-seeking bureau is as likely to become inefficient as it is productive. Indeed, only the extreme myopia of deterministic adaptation made the agency of Version 1 wind up at the pluralist equilibrium: the agency in Version 2 is far better off.

### Version 4: The Rate of Adaptation

The superstitious learning of Version 3 introduced the idea that the power characteristics of the bureau's environment are mediated by institutional structure (the legislature's instruments) and by the agency's adaptive strategy. Unlike a simple pressure model, in our model the power of interest groups, though important, is not conclusive. Version 4 explores the effects of a second property of adaptation: the speed of adjustment.

A budget-maximizing agency is by definition indifferent to policy and therefore has no intrinsic desire to lower enforcement. Yet in Version 3, though consumers outspent firms, the agency drove enforcement to zero. Because enforcement = efficiency · budget, the agency was reducing efficiency faster than the legislature was increasing the budget. This observation suggests that the agency's rate of adjustment is crucial. Version 4, keeping everything else constant, introduces an agency that adapts more slowly. Table 1 records the effects of this change on the equilibrium enforcement, budget, and efficiency. Because Congress pumps in funds faster than the bureau becomes unproductive, enforcement reaches and temporarily passes the pluralist equilibrium. When this occurs, the legislature reverses its practice of granting it larger budgets, and the agency for the first time gets negative feedback suggesting that still lower efficiency is unwise. The eventual result is the pluralist equilibrium.

There are two important lessons here. First, the pluralist equilibrium of Version 1 is more than an accidental by-product of superstitious learning. The inertia introduced in Version 4 ultimately destroys the agency's benign environment and requires that it make choices in the face of negative feedback. That it settles down at the pluralist equilibrium, rather than somewhere else, indicates that there is something truly general about this particular level of enforcement. The second lesson is that inertia performs a systemic function of real importance in this version of the model. Because bureaucratic inertia is ritually condemned in both popular and scholarly accounts, this is an intriguing result that underlines the value of further inquiry into its positive aspects.

### Version 5: Oversight

Clearly the consumers are handicapped by the paucity of instruments available to the legislature. The sole response in Versions 2 and 3 to declining enforcement is to throw more money at the bureau, but this only teaches the agency to become still less productive. In the real world, however, such a vicious spiral could not continue indefinitely, for society would eventually be devoting all of its resources to regulation, and such a system is not viable. Systems that survive, therefore, must have mechanisms for checking these explosive forces. One such mechanism is taxes. We will turn to this shortly. Here we focus on a second mechanism, legislative oversight.

Specifically, Version 5 of the model endows the legislature with an oversight capacity to complement its budgetary authority, and assumes in addition that the bureau dislikes the hassling that oversight entails. Note that the latter condition is necessary if oversight is to have any impact on bureaucratic behavior. A bureau insensitive to hassling will simply ignore the legislature's complaints. Although this may certainly happen empirically (in fact, legislatures probably feel it happens all too often), it means that oversight would be superfluous to the model.

In keeping with the model's overall theme that decision makers rely on simple heuristics, the oversight mechanism is exceedingly crude. It is activated whenever two events occurred in the previous period: first, the legislature gave the agency a bigger budget; second, enforcement declined. Although we assume that legislators do not have a good grasp of the agency's internal affairs, they do hear about the enforcement level from the interest groups, and of course they know what the budget is. These two pieces of information suffice to yield an inference of mismanagement whenever the agency produces less output with more money. In such instances, the legislature hassles the agency with time-consuming hearings, bad publicity, threats to bureaucratic careers, and similar measures.

This mechanism is crude, but it turns out to be surprisingly potent. As we saw in Version 2, an agency that likes slack and budgets creates the degenerate solution: an explosive budget with efficiency and enforcement moving to zero. Version 5 takes the same context and introduces oversight. Simpleminded and myopic though it is, episodic legislative hassling has a striking impact on the process of bureaucratic adjustment. The agency is no longer in a benign environment: as it moves toward the degenerate solution, the combination of bigger budgets and drops in efficiency produce mounting sanctions in the form of hassling, until finally the costs of greater inefficiency outweigh the benefits. The bureau then reverses course. After a series of adjustments and counteradjustments, it winds up at the pluralist equilibrium. Thus, legislative oversight interrupts the process of superstitious learning and saves the system from the degenerate solution, producing the same equilibrium level of enforcement as Version 1's budget-maximizing model—although, owing to its circuitous time path, at a higher budget and lower level of efficiency.

Oversight does not produce socially optimal behavior, nor does it guarantee legislators the best of all possible electoral worlds.[19] It does, however, perform a crucial systemic function that puts a rein on the agency and allows regulation to "work." This occurs even though the legislature knows virtually nothing about bureaucratic costs, performance, or efficiency, and even though it reacts only on those occasions when the evidence of inefficient performance is painfully obvious.

The impact of oversight also helps to illustrate the special status of the pluralist equilibrium. We have presented five runs of our model now; two have produced the degenerate solution, three the pluralist equilibrium. In fact, it is a general property of our model (in the absence of taxes and agency policy preferences) that these are the only two solutions. The pluralist equilibrium, therefore, appears to be the natural equilibrium of a stable system, for it is the only solution compatible with the survival of the system as a whole.

## Version 6: Agency Policy Preferences

In their motivational assumptions for bureaus, the standard formal models have clearly been influenced by the analogy of profit- or revenue-maximizing firms. Hence the emphasis on budgets and slack. But although it is ordinarily true enough that firms are not directly motivated by output, this is a less plausible assumption for bureaus, for there is good evidence that bureaucratic officials are often motivated by policy preferences (Aberbach & Rockman, 1976). The battles between liberal career civil servants and conservative political appointees in the Nixon and Reagan administrations are hardly battles over budgets and slack. They are battles over policy, over different notions of the "ideal" levels and directions of bureaucratic performance.

To isolate the effect of policy preferences, Versions 6A and 6B introduce agencies that care only about policy. We assume throughout that the agency's preferences are those of its dominant coalition, and that the composition of the dominant coalition is strongly influenced by presidential appointments. Although a more comprehensive model would surely include the president in an explicit fashion, this at least gives us an indirect opportunity to investigate whether presidents can shape regulatory performance by, in effect, changimg some of the parameters of the system. In a regulatory context where firms oppose consumers, it is reasonable to categorize the agencies as either probusiness (perhaps reflecting appointments by a Republican president) or proconsumer (perhaps due to Democratic appointments). Abstractly, we consider an agency to be probusiness if its ideal enforcement level is less than the social optimum, as proconsumer if its bliss point exceeds the social optimum. To minimize ambiguity, we proceed by comparing a probusiness agency whose ideal level of enforcement is 100 (smaller than both the social optimum of 150 and the pluralist equilibrium of 184) with a proconsumer agency whose ideal point is 300 (larger than both).

Not surprisingly, a probusiness bias leads to less enforcement than the pluralist equilibrium. Seeking to reach its optimal enforcement of 100, the bureau tends to diminish its efficiency. However, the agency's success in reducing enforcement mobilizes consumers, which ensures a proconsumer legislature. Thus ensues an odd conflict between the two branches: the legislature pushes money on a reluctant bureau, which combats the added revenues by further decreasing its efficiency. The legislature responds by giving still more money, and the pattern continues. The result is that enforcement cycles in a compromise region bounded by the agency's ideal policy position and the pluralist equilibrium. As enforcement cycles, budgets eventually explode, and efficiency plunges virtually to zero.

Thus, a characteristic feature of a system where the pluralist equilibrium exceeds the agency's bliss point is conflict between the legislative and executive branches. (Our simple model does a fair job of simulating, for example, the short-term recent conflict between Congress and the EPA.) What happens if the agency is proconsumer? At first blush, it appears that a proconsumer bias should produce a result symmetric to the probusiness bias: enforcement would equal a weighted average of the pluralist equilibrium and the agency's bliss point, with ongoing struggle between the legislature and the agency. Yet this is wrong. It reflects an extension of simple power reasoning that is inappropriate given the institutional structure of the model. As Version 6B suggests, the agency's decision variable, efficiency, cannot drive enforcement permanently higher than the pluralist equilibrium. The agency's policy preference has no significant effect. Jacking up productivity briefly augments enforcement, but this disproportionately mobilizes business, shifting the legislature to a budget-cutting posture. Adjustments and readjustments eventually restore the pluralist equilibrium.

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[19] As one example of its suboptimality, note that the oversight strategy permits declining productivity if output is rising. Because this can happen only when the budget is increasing, this toleration represents systemic slack: when the budgetary climate is good, for either macroeconomic or political reasons, hierarchical control loosens. There is a strong similarity here to the dynamics of slack accumulation in the behavioral theory of the firm (Cyert & March, 1963).

To understand the agency's impotence more clearly, imagine that the agency simply sets efficiency equal to 1 and keeps it there. It would then be doing its utmost to reach its ideal point, but its utmost is inadequate. The legislature controls the budget, and legislators will be induced by the parity of interest-group campaign contributions to cut the budget until enforcement equals the pluralist equilibrium.

The asymmetrical impacts of proconsumer and probusiness policy preferences result from the distinctive manner in which the legislative and bureaucratic control mechanisms interact. The legislature can force a budget on a probusiness agency, but it cannot force the agency to spend all (or any) of it on enforcement, for the choice of efficiency is up to the agency alone. Thus, there is a constant struggle giving rise to the compromise region. On the other hand, the legislature can respond to the expansionist designs of a proconsumer agency by cutting its budget, and the agency can only fight back by increasing its efficiency. Once the agency reaches perfect efficiency, budget cuts are fully translated into declines in enforcement, and the agency can do nothing about it. Thus, the pluralist equilibrium.

It is a bit strong to say that a proconsumer bent has *no* influence: agencies that try to drive enforcement beyond the pluralist equilibrium become highly efficient. This result has a curious implication for antiregulation interests: if these interests faced a choice between a proconsumer agency and a budget seeker disciplined by oversight, they should prefer the zealous bureau. The explanation for this counterintuitive preference is that, owing to the power configurations of the bureau's environment, both agencies will wind up producing the same amount of enforcement, but the proconsumer agency will cost less. Thus a realistic assessment of power dynamics should make antiregulation forces look more kindly upon career bureaucrats who singlemindedly support their opponents.[20]

### Overview: Basic Types of Solutions of the Model without Taxes

Although we have thus far analyzed only seven versions, the reader has been introduced to all the basic types of solutions attained by our much-more-extensive experimental manipulation of the system. Despite the combinatorial complexity of alternative goals, adaptation rates, congressional instruments, and initial values, all the versions reach one of three outcomes: the pluralist equilibrium, the degenerate solution, or the compromise region.[21]

Analyzing the versions within each type strengthens the inferences made on the basis of pairwise comparisons. Consider first the degenerate versions. With only one exception, all runs producing the degenerate solution were characterized by three properties: the agencies like slack, they adapt quickly, and Congress lacks oversight. When the three properties are present, the agency's policy preferences are irrelevant, as is budget-seeking.[22] Normatively, the combination of a slack-seeking bureau and a legislature that does not monitor is unfortunate. Empirically, we believe the combination to be rather rare.

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[20] In real bureaucracies, policy goals usually comingle with less lofty objectives. What happens when the agency seeks other goals—budgets, slack, and avoiding oversight—in addition to policy? The three familiar patterns are recreated. Adding slack to the agency's motivation recreates the exploding pattern of Version 2: huge budgets, abysmal inefficiency, little enforcement. Adding budgets is less damaging. The budget seeking probusiness agency merely cycles closer to its ideal point, and the proconsumer agency is basically unaffected. Thus, slackseeking and budgetseeking differ more than is usually believed. Because revenues are allocated either to slack or output, a preference for more perquisites directly impairs efficiency. Budgetseeking lacks this directly corrosive effect. Finally, applying oversight to a slackseeking proconsumer bureau restores the pluralist equilibrium; overseeing a slackseeking probusiness agency reproduces cycling in the compromise region. (The authors will provide a complete table of results upon request.)

[21] Although one cannot prove that a simulation model will remain confined to a given pattern for all parametric combinations, extensive experimentation makes us confident that realistic parametric settings result in one of the three solutions.

[22] We should note that these statements pertain to "on-off" versions of policy preference, that is, certain parameters are set equal to 1 (on) or 0 (off). One can increase the relative importance of any goal by increasing a parameter's value from 1 to, say, 3. We experimented with these possibilities, and found that no new types of solutions appeared. Sometimes, however, a version would switch from one type to another.

All of the versions attaining a compromise solution share one property: a probusiness policy preference. No other attribute much mattered. If the agency lacked a business orientation, no combination of the other parameters would drive it into the compromise region; if it had it, there were few combinations that would move it out of the region.

Four patterns characterize the pluralist equilibrium category. First, most proconsumer agencies reached the pluralist equilibrium. The only ones failing to do so liked slack and were not overseen. Second, of the bureaus indifferent to policy, all those disciplined by the crude oversight procedure reached the pluralist equilibrium. Third, the rate of adaptation matters. Surprisingly, fast learning is not always a blessing, nor slow learning always a curse.[23] Several degenerate versions are transformed into pluralist equilibrium solutions when they adapt slowly rather than quickly. A slow adjustment rate means that Congress is increasing the budget faster than the bureau is decreasing productivity. The net result is increased enforcement, indicating that the legislature can push on a string—if it pushes fast enough and is willing to pay the price.

Fourth, although all the pluralist equilibrium versions produced the same amount of regulation, they did so with varying degrees of efficiency. Of those that were most efficient, not one both sought slack and did so with impunity. There were some bureaus that liked slack, but in all these cases the legislature monitored it. Finally, many more proconsumer than probusiness agencies are very efficient pluralist equilibrium versions.

## The Final Step: Introducing Taxes

It has been convenient thus far to assume that firms and consumers ignore taxes, treating governmental policy as free goods or bads generated by the bureau. There are empirical grounds for using this as a starting point. The budgets of regulatory agencies are typically small compared to either the national budget or the social costs and benefits of regulatory policy. In such contexts a model without taxes is a reasonable approximation. There are also important analytical grounds. This approach has allowed us to develop a theoretical foundation that highlights the basic forces of the system—forces that, it turns out, are constrained and masked by the role that taxation ultimately plays. By waiting until the final step to introduce taxation, we can better understand the more general model and the distinctive contributions of its parts.

Taxes might be added to the model in different ways. To keep matters simple, we assume that all budgets are financed by taxes and that all taxes are paid by consumers. Little is lost by assuming that firms escape taxation, since they already oppose regulation anyway. The interesting effects are on consumers.

Formally, these effects derive from a change in how consumers calculate the benefits of enforcement: we now assume that the value consumers ascribe to regulation equals the benefits of regulation minus its tax costs, where taxes just cover the agency's budget. What they receive they must now pay for. As a result, how much consumers value any level of enforcement depends on the budget required to produce it, and therefore on agency efficiency. As efficiency increases, the required budget decreases, taxes decrease, and consumers gain. More generally, the consumer benefit function shifts every period as the agency chooses new efficiency levels. It has become an endogenous component of the model.

This has two far-reaching consequences. First, because the consumer resource function is anchored in the consumer value function, it no longer describes a fixed relation between enforcement levels and resource contributions. It, too, is dynamic. As agency efficiency increases, the resource function shifts upward, reflecting greater consumer value and support of agency efforts at each enforcement level. Second, because the consumer resource function is now dynamic, there is no longer a unique point at which the consumer and business resource functions intersect. That is, there is no longer a single pluralist equilibrium. Instead, as illustrated in Figure 3, shifts in the consumer resource function define an infinite number of pluralist equilibria, each representing a potential balance of power for the system. Thus, although the meaning of the pluralist equilibrium remains unchanged, it is no longer an exogenously determined point to which the system gravitates. The pluralist equilibrium itself has become endogenous.

What can we say about political behavior in this new world of taxpaying consumers? Following the same logical steps as before, computer simulation indicates that this is one of those fortunate cases in which adding complexity creates simpler, more unified patterns of behavior. The salient transformation is that the degenerate solution and the compromise region of our earlier model now vanish—with rare exceptions, all system outcomes

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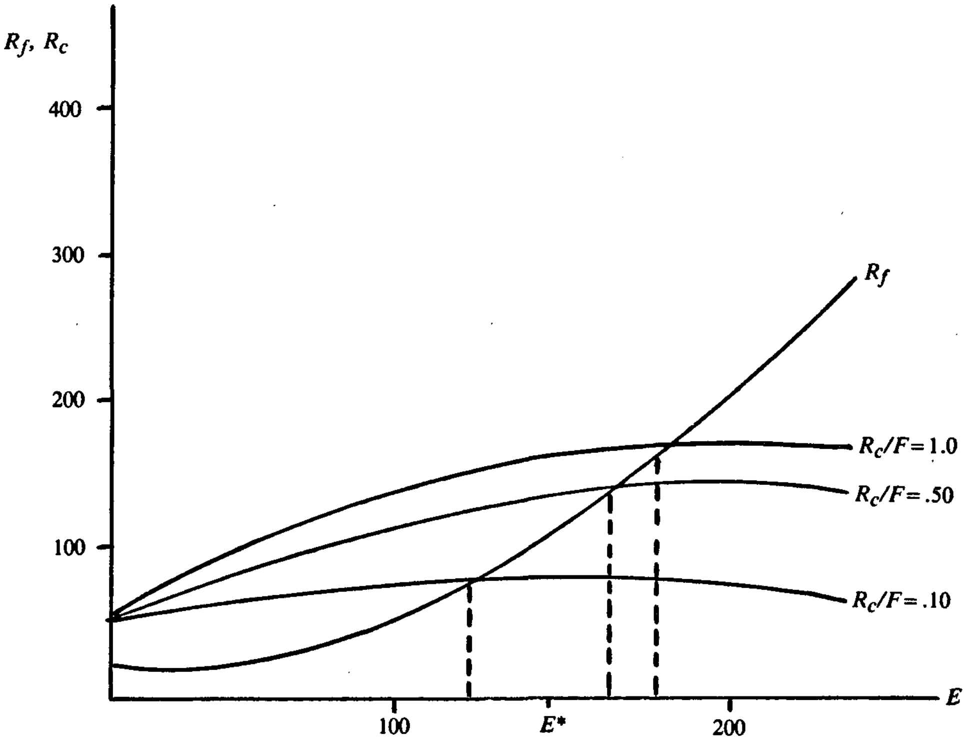
[23] The seemingly paradoxical benefits of slow learning have been noted before: see, for example, Lave and March (1975).

**Figure 3. Pluralist Equilibria in the Tax Model**



*Cost and Benefit Functions (not graphed)*

Costs $= .023E^2$
Benefits $= 20E - .04E^2$ – budget

*Resource Functions*

Firms $R_f = 10 + .20\ (.023E^2)$
$= 10 + .20$ (costs)

Con. $R_c = 50 + .05\ (20E - .04E^2$ – budget)
$= 50 + .05$ (benefits)

$E$ = Enforcement
$F$ = Efficiency
$E^*$ = Social Optimum
$R_f$ = Political Resources of Firms
$R_c$ = Political Resources of Consumers

are pluralist equilibria.[24] What we have is a very stable balance-of-power system.[25]

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[24] Of course if the slopes and intercepts of the resource functions were significantly altered, the system would not necessarily reach a pluralist equilibrium. With resources continuing to be linear functions of convex costs and concave benefits, there are two cases to consider. First, if the firms' resources exceed the consumers' at all enforcement levels, then the system would obviously not attain a pluralist equilibrium. Instead, no enforcement would be produced. Second, if firms outmobilize consumers at very low and very high enforcement levels, but the consumers outmobilize firms at intermediate quantities, there are two locally stable outcomes: the interior solution of the pluralist equilibrium and the corner solution of zero enforcement. In this case the more diffuse group has to overcome a mobilization threshold in order to sustain the program.

[25] The exceptions are odd cases of persistent instability. One is a proconsumer agency that values slack, which fluctuates indefinitely. Experimentation with this case shows that if policy is weighted a bit more or slack a bit less in agency utility, equilibrium is reached. Thus, here and elsewhere, instability is the exception, not the rule. Moreover, in contrast to the nontax model's

To see why the degenerate solution disappears, consider an agency that seeks both slack and budgets. In the taxless model, this bureau decreased its efficiency relentlessly. Because it reduced enforcement, consumers were more mobilized than firms, inducing legislators to pump up the bureau's budget. Rising revenues taught the bureau that sloth pays. It therefore became still less efficient, output fell, legislators pumped in more money, and the cycle continued, leading to exploding budgets and zero efficiency. But in the tax model, consumers recognize the cost of the bureau's profligate ways: their support for the agency diminishes as efficiency declines, thus giving legislators less incentive to add budgetary fuel to the bureaucratic fire. Eventually, the alienation of support becomes so large that the bureau can no longer gain from reducing efficiency, and it reverses course, ultimately settling down—with the rest of the system—to a stable balance-of-power equilibrium.

More generally, the forces that earlier produced the degenerate solutions of zero efficiency, zero enforcement, and infinite budgets are constrained by the incentive-effects of taxation, which induce consumers and therefore legislators to put on the brakes. The result is hardly an absolute blessing for consumers, since enforcement and efficiency are low and budgets are much higher than necessary. But the system is in stable equilibrium, positive levels of enforcement are achieved, consumers do realize net benefits on the exchange, and all of this is far preferable to the degenerate solution.

Similar logic explains why the compromise region vanishes in a world with taxation. Recall that the compromise region arose because of a policy difference between a probusiness agency and the induced preference of the legislature, which is striving (in effect) toward the pluralist equilibrium. The agency, trying to reach its probusiness bliss point, reduces efficiency; the legislature responds by throwing money at it, which produces a see-saw contest in which enforcement cycles within a region bounded by the agency's bliss point and the pluralist equilibrium, accompanied by plummeting efficiency and exploding budgets. When consumers internalize taxes, however, the legislature finds itself at a disadvantage in its struggle with the agency. The legislature's crucial weapon is now treated as a cost by consumers, who will not long support throwing money at the agency. Budgetary increases diminish the consumer resource function, shifting the pluralist equilibrium to the left. The compromise region therefore contracts—but, because the agency's bliss point is fixed, this contraction is entirely due to a movement of the pluralist equilibrium toward the agency ideal. The net effect of these new forces is to destroy the compromise region, along with the rising budgets and declining efficiency that upheld it. It is replaced by a stable equilibrium very near the agency's bliss point. For a probusiness agency motivated purely by policy, efficiency is very low (.07) and enforcement is virtually right at the agency's ideal (101). But all the relevant values are quite stable, and balance of power prevails in the environment.

As in the degenerate case, then, the forces creating the compromise region continue to operate: the struggle between the agency and the legislature is underpinned by an explosive potential for infinite budgets and zero efficiency. But this potential is constrained by the disciplining role of taxation, which saves the system from budgetary disaster—and, in the process, creates a context in which business firms are better able to get what they want from government.

Aside from the elimination of the degenerate solution and the compromise region, the tax model is virtually identical to our earlier model in its implications for the system's outcomes. Virtually all outcomes from the tax model are now pluralist equilibria, whose relative values clearly reflect the familiar operation of the same basic set of forces, that is, slack, oversight and so forth have the same effect as before.

## Discussion

In this article we have proposed a general framework for pursuing a theory of bureaucratic politics. By design, it incorporates a range of components that more conventional models of bureaucratic politics have simplified away, components that are fundamental to the dynamic interaction among bureaucrats, politicians, and interest groups. In surface respects it cannot avoid being highly complex, and it may appear for that reason to fall into the trap that models of the neoclassical variety have tried to avoid. Yet, as our analysis has shown, its essential property is not complexity but simplicity—and it imposes coherence on bureaucratic politics by drawing upon and integrating the complexity inherent in the substantive context, not by assuming important aspects are irrelevant. Among its most general implications are the following.

1) Under a wide range of conditions, the regulatory system tends toward the pluralist equilib-

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degenerate solution and compromise region, the unstable cases in the tax model are not explosive: all variables cycle within reasonable bounds. Even its instabilities are stable by comparison.

rium.[26] The logic of adjustment is very much a reflection of traditional pluralist thought. As Truman (1951) argued long ago, mobilization begets countermobilization, and changes in the underlying balance of power among social groups are translated by political institutions into corresponding changes in policy outcomes.[27] Due to this process of negative feedback, success in this kind of policy sector is self-limiting.[28] As we have shown, moreover, this result is entirely consistent with—indeed, to be expected from—rationally adaptive behavior, and it holds even though bureaucratic and legislative participants play distinctive, self-interested roles in creating policy from social inputs. The common notion that rationality and the translation effects of institutions are somehow inconsistent with pluralism, then, is overdrawn. Although more elaborate versions of our model (incorporating, for example, legislative committees) may point to conditions that imply alternative expectations, there is every reason at this stage to stress the contribution that pluralist ideas can make to our understanding of bureaucratic politics, as well as their compatibility with more "modern" lines of analysis.

2) This equilibrium is reached in a groping fashion by decision makers who only dimly understand the effects of their own actions. Thus, it is highly consistent with the descriptive literature on policymaking, which emphasizes the complexity of the policymaking environment, the frequent missteps, the unintended consequences, as well as the "disjointed incrementalism" of policymaking in a polyarchical system (Dahl, 1971; Lindblom, 1965). Indeed, in many ways the model is a formalization of the "intelligence of democracy." With Lindblom, we attribute the intelligence (what there is) of policy outcomes to properties of the system rather than to the brilliance of individuals. Our actors are not brilliant. Like the decisionmakers in the "Science of Muddling Through" (Lindblom, 1958), our actors ignore side effects, consider only a limited range of alternatives, and adapt as best they can to a complex environment. Through the feedback and mutual adjustment occasioned by their interaction, they tend collectively to produce coherent, sensible outcomes that the individuals themselves neither intend nor have the power to bring about.[29]

3) The system is not socially optimal—but the direction of suboptimality differs from what the critics of pluralism have claimed. Since Olson's (1965) work first appeared, it has been common to emphasize the advantages of compact groups over diffuse groups and to explain the suboptimality of policy outputs via this disparity. In our model, this logic would imply a definite bias in favor of business and against consumers, and thus an equilibrium level of enforcement lower than the social optimum. Yet this argument is not in general true, and for the specific model of this

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[26] Is the pluralist equilibrium the same solution that completely rational decision makers would reach? This is an important question, both normatively and empirically, but it has no easy answer. Part of the problem is that there are many ways to specify an optimization model. Analytical choices must be made regarding, for instance, the information available to each actor, how they formulate beliefs including expectations about the future, the nature of their decision rules, for example, maximize expected utility or minimax loss, and how the actors themselves are conceptualized, for example, are the interest groups unitary rational actors, coalitions, or sectors of decentralized decision makers? Obviously, different choices along these dimensions could easily produce different systemic outcomes, so we cannot say in general what a shift from adaptation to optimization might imply. We can, however, offer two specific conjectures. First, if the situation were modelled as a cooperative game—that is, actors can costlessly make binding agreements—with the interest groups taken as rational actors, then we conjecture that if an equilibrium exists, it is to produce the socially optimal amount of enforcement (Coase's theorem). Because in general the pluralist equilibrium is not equal to the social optimum (see text, below), the adaptive model does not converge to the equilibrium attained by these rational agents. Second, if the situation were modelled as a noncooperative game among perfectly informed legislators, with the interest groups and the agency taken as passive reaction functions rather than as strategic agents, then we conjecture that the pluralist equilibrum is the Nash equilibrium of the game. (We would like to thank an *APSR* referee for suggesting this conjecture.) More generally, however, computing the equilibria reached by optimizers in a multiperiod world of the sort represented here would be very difficult—indeed, a fullscale project in its own right.

[27] Here, as in virtually all dynamic models, the parameters are held constant in order to investigate the system's long-run tendencies. Because empirically these coefficients may change, one may never observe a regulatory system in equilibrium. Instead, it may always be moving toward equilibrium.

[28] We must point out that we have assumed that the regulatory policy system is not so important that losing is debilitating. In such a world, the positive feedback of the Matthew effect—"To him who hath shall be given"—creates an unstable system of cumulative advantages (Dahl, 1971). The mobilization of previously unorganized interests such as minorities and environmentalists indicates that the American system is described more by negative than by positive feedback (Landau, 1973).

[29] Indeed, it is intriguing to note that this system of myopic and adaptive decisionmakers may be converging to the same outcome that would be attained by perfectly informed and perfectly rational legislators engaged in a noncooperative game, as conjectured in note 26.

article it is quite false. The reason turns on the difference between the pluralist equilibrium and the social optimum. Enforcement generates costs and benefits for society, and its level is socially optimal when net benefits are maximized, that is, when the marginal benefits and marginal costs of enforcement are equal. The pluralist equilibrium occurs, however, at the enforcement level for which the political resources of consumers and firms are equal. There are accordingly two crucial dimensions of difference between the two solution concepts. First, the social optimum is based on the cost and benefit curves themselves, whereas the pluralist equilibrium is based on the political resource curves. Second, the social optimum is based on marginal curves, the pluralist equilibrium on total curves. The combination of these factors implies that the pluralist equilibrium may be above or below the social optimum.[30] Since the Pluralist Equilibrium is the "natural" policy outcome of our system, this means that regulatory enforcement may often be above the social optimum, even though consumers are at a distinct organizational disadvantage relative to business. In the model presented here, this is precisely what happens: business is assumed to be four times more effective at translating values into resources, but the pluralist equilibrium usually lies above the social optimum anyway.

4) The power of bureaus to get what they want has been exaggerated. Although bureaus can sometimes move to a paradise of exploding budgets and slack, they cannot do so when checked by even a primitive form of legislative oversight, nor when their adjustment is slowed by inertia, nor when their constituents are aware of the burden of taxes. In the most general case they wind up at the pluralist equilibrium. There, budgets and slack may vary depending upon the time path of the process, but the agency has clearly been "led" by the system's logic of adjustment and has not engineered its preferred bureaucratic outcome. When the agency has policy preferences, its influence is more apparent—but it is asymmetrical, and it only works to produce smaller levels of enforcement than critics lead us to expect. Specifically, a probusiness agency can move the system toward its own ideal point, increasing its budget and slack in the bargain. A proconsumer (and therefore expansionist) agency, on the other hand, is helpless to prevent movement to the pluralist equilibrium.

At a more aggregate level, these conclusions provide a different perspective on the growth of government. In the formal modelling literature on this matter, we have seen the blame for big government assigned to different institutions. First, the bureaucracy did it (Niskanen, 1971). Next, Congress did it (Fiorina, 1977). Now, we argue that the routine functioning of a pluralist system is responsible for the size of government. Outcomes are collective by-products of the decentralized choices of several institutions, not the unique responsibility of any single one. And there is no uniform bias toward bigness—government may often be too small rather than too large.

5) The legislature and, indirectly, consumer groups face a fundamental dilemma in the use of control mechanisms. By cutting budgets, the legislature can effectively prevent an agency from producing "too much" enforcement. But it does not work the other way round: a probusiness agency producing "too little" can absorb a larger budget by increasing inefficiency. Moreover, because the budget is an incentive in itself as well as a means of generating output, a policy-indifferent agency may view increased budgets as rewards for inefficiency and low production. If the agency's constituents are not cost-conscious, this view of budgets will prompt the bureau to move toward the degenerate solution—just what consumers seek to avoid. Fortunately, even if constituents ignore taxes, the legislature has a second mechanism—oversight—which plays a crucial role in saving the system from explosive budgets and inefficiency. The wonder of oversight is that it requires so little of the legislature and nonetheless works so well in reversing systemic pathologies.

6) Bureaucratic inertia can be good for the system. Although this seems odd, given the bad reputation inertia has acquired, it actually makes sense: for inertia constrains the flexibility of bureaus in their pursuit of ever-larger budgets and slack, inhibiting their adaptation.

7) The implications of the model pose distinct problems for liberal supporters and conservative opponents of regulation. For liberals, the dilemma arises because the peculiar asymmetry of the budget mechanism operates to their disadvantage. A conservative majority can achieve lower enforcement quite effectively by cutting the budget, but a liberal majority is far less able to engineer higher enforcement by increasing the budget, and its efforts can easily backfire by rewarding agencies for poor performance. Conservative opponents face a different dilemma: a probusiness agency is able to triumph over a proconsumer legislature, but only at the cost of gross inefficiency. They are thus torn between the com-

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[30] To clarify further, that the social optimum derives from marginal cost-benefit curves whereas the pluralist equilibrium derives from total resource curves tends to make the latter solution exceed the former—yet the greater the relative advantage of business in mobilizing resources, the farther to the left the two resource curves will intersect, and the smaller the pluralist equilibrium will be. Either force may predominate.

peting desires for less regulation and more efficient government.

## Conclusion

This model is just a beginning, for we purposely ignore institutional and strategic aspects of policymaking that must eventually be part of a satisfactory theory. The task for the future is to move toward more elaborate models that take these additional aspects systematically into account. In our view, one of the real advantages of the general framework we have developed here is that it easily accommodates efforts to move in a variety of interesting directions, thus encouraging the proliferation of a family of models whose similarities and differences may well provide important new insights into bureaucratic politics. The following are but a few of many elaborations that seem to us both feasible and promising.

**Legislative Committees.** Legislatures can be assumed to vote only on alternatives generated by committees, which are therefore in a position to use their agenda control to shape outcomes for the legislature and, less directly, for the system as a whole.

**Electoral Districts.** Legislators can be assumed to be elected from geographically distinct districts, with the distribution of group costs and benefits and the ensuing balance of group resources varying from district to district.

**The President.** The role of the president can be explored not only by means of the appointment mechanism but also by introducing an OMB with budgetary and monitoring powers and perhaps by incorporating the president's veto power.

**Interest Group Strategy.** Groups can be allowed to adopt various strategies in their efforts to influence outcomes, for example, contributing only to supporters (not against opponents), basing all contributions on the distance of the legislators from the median, or rewarding opponents if they move in the right direction.

**Bureaucratic Innovation.** Agencies can be allowed to devote a portion of their budgets to the generation of new programs. These innovations would then alter the production of enforcement or its consequences for business and consumers.[31]

**Modes of Adaptation.** Decision makers can be assumed to adapt in more sophisticated ways to their environments. Among other things, this may involve probabilistic adaptation or the development and modification of belief structures.

Whatever elaborations may be introduced, they will be unified by a common framework, a macro-theory of politics based on the microfoundations of bounded rationality. This theory, particularly its formal representations, stands to provide new insights into the nature of bureaucratic politics by placing Simon's "administrative man" (1947) squarely in the context of the larger political system. It also stands to illuminate longstanding issues concerning the intelligence of democracy. For decades political scientists have wondered whether voters are too ill-informed, or politicians insufficiently rational, for the overall health of polyarchies. The classical response of pluralist thinkers to these concerns is that, although we cannot depend upon individual decision makers, we can rely upon properly designed systems that "pit ambition against ambition" and correct for the myopia and ignorance of its constituent elements. Our framework provides a formal means for systematically investigating these issues.

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[31] For work that moves in this direction, see Levinthal and March (1982).

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# Vulnerabilities and Responsibilities: An Ethical Defense of the Welfare State

ROBERT E. GOODIN
*University of Essex*

*We all acknowledge strong special responsibilities towards families, friends, clients, and compatriots. The moral basis of these responsibilities is traditionally analyzed in terms of self-assumed obligations. That analysis substantially restricts their scope, because we voluntarily commit ourselves to only a limited range of people. In this article I argue that it is the beneficiary's vulnerability rather than any voluntary commitment as such on the part of the benefactor that generates these special responsibilities. This analysis provides an argument for broader notions of responsibility, because there are many more agents vulnerable to us (individually or collectively) than to whom we have made commitments, in any sense. The welfare state is one particularly apt way of discharging at least some of these further responsibilities. Unlike more individualized responses, the welfare state can satisfy the criteria of a morally acceptable dependency relationship.*

The aim of this article is to broaden our sense of social responsibility. It is first and foremost an argument in favor of state welfare services. The basic strategy is to try to put them morally on a par with the services we render—and which we firmly believe we *should* render—to family and friends. The method of discharging these other responsibilities differs but, I will argue, their moral basis is the same. A parallel argument, which here I have space only to sketch baldly, generates analogous international and intergenerational responsibilities and responsibilities for protecting animals and natural environments. Thus this approach promises to provide a unified analysis of a great many of our moral responsibilities.

The argument employs the method of the "reflective equilibrium," in which the goal is to bring our general moral principles and our "settled intuitions" about what is right and wrong in any particular case into line with one another, striving all the while for coherence within our set of general principles and for concordance between those principles and our various other "background theories" (Daniels, 1979; Rawls, 1971, sections 4 and 9).

In such a procedure, we must always be prepared to reject some of the moral duties and responsibilities that untutored intuition lays upon us. By the same token, the reflective equilibrium can also carry us well beyond ordinary moral instincts by showing us that we have moral duties and responsibilities that intuitively we might deny having. This is the side of the reflective equilibrium that interests me here.

The moral theory that best explains those intuitions that we do have will, typically, also commit us to moral judgments about what we should do in situations that are strictly analogous but about which we have no strong intuitions. By showing us what lies hidden within our ordinary moral sentiments, the reflective equilibrium can thus drive us to acknowledge commitments we would preanalytically have shunned.

## Special Responsibilities

My argument starts from what I take to be one of our firmest moral intuitions, one that we would be loath to sacrifice in this process of reflective equilibrium. That is the intuition that we have especially strong responsibilities toward our families, friends, clients, and compatriots. Charity, we seem strongly to believe, not only does but *should* begin at home. Our primary responsibilities are toward those who stand in some special relationship to us. Strangers get, and are ordin-

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arily thought to deserve, only what (if anything) is left over.

An example from Bernard Williams effectively evokes this intuition. Suppose you were in a fiery air crash and able to pull out only one other victim before the entire plane exploded. On one side of you is a distinguished surgeon, black bag at hand, who could save the lives of several other passengers thrown clear of the airplane when it went down. On the other side is your son. It would, according to the intuition Williams shares with so many others, be morally reprehensible for you to save the surgeon in preference to your own son.[1]

Now, of course, such favoritism would be perfectly understandable from any number of perspectives. Both psychologically and sociologically, it is easy to see why our affections should extend principally to those "of our own kind" rather than to the unknown strangers the surgeon might save. Sociobiologists would point out that saving your son increases the chances of your own genes surviving in the next generation. There is no shortage of adequate explanations for why you would, in fact, favor your own flesh and blood. The question is whether morally you should.

A long tradition in moral theory says firmly you should not. Indeed, any universalistic theory—be it Kantian or utilitarian—would take just that stand. But that, say many philosophers and even more laypersons, is precisely what is wrong with all universalistic ethics. Sir David Ross (1930, p. 21) insists that "the essential defect of the . . . utilitarian theory is that it ignores . . . the highly personal character of duty"; he is outraged that, for example, act-utilitarians are incapable of distinguishing between my giving $10 to my benefactor and my giving the same sum to someone who needs it just as badly but "to whom I stand in no special relation." In a continuing stream of articles, Williams (1973, p. 110; 1981) has been arguing for the moral importance of respecting people, which entails among other things respecting their "projects," including their special commitments to their families and friends. For Parfit (1984, p. 95), "common-sense morality largely consists" in "special obligations" owed to "the people to whom we stand in certain relations—such as our children, parents, pupils, patients, members of our own trade union, or those whom we represent." Or, for a final example, a central theme in Walzer's (1983, p. 31) *Spheres of Justice* is that, "The idea of distributive justice presupposes a bounded world, a community within which distributions take place, a group of people committed to dividing, exchanging and sharing, first of all, among themselves."

Our firm moral intuitions go further still. Not only do we feel that we have these special duties to family and friends, alongside our other, more general moral duties. We also feel that "carrying out these obligations has priority over helping strangers," as Parfit (1984, p. 95) goes on to say. Perhaps those special obligations are not quite strong enough to override negative duties not to harm others. It would be thought wrong for us to commit murder, even if that were necessary for us to feed our own children. But apparently those special duties are quite strong enough to override any duties we might have to render positive assistance to others in general. In *Bleak House,* Dickens ridicules Mrs. Jellyby's "telescopic philanthropy," that is, her tendency to aid distant Africans while neglecting her own family. No doubt most of us are intuitively inclined to say that it is right to satisfy our own children before giving food to needy neighbors. The consequence of acting on such intuitions, however, is that our duties to those with whom we enjoy some special relationship ride roughshod over our duties to help others at large. That, obviously, is an outcome that any theory of social justice, or even of simple humanity, would dearly hope to avoid.

In what follows, I shall *not* be arguing that we lack any such responsibilities toward family and friends. Neither shall I be denying the strength of such claims. What I shall be arguing is that there is *nothing special* about those responsibilities. There are many others with precisely the same basis—and, depending on circumstances, perhaps even the same strength—as those responsibilities that we have always regarded as particularly binding. The upshot of this argument, if it is successful, is that we are not justified in our present practice of serving one set of claimants systematically to the exclusion of the other.

## Alternative Bases of Special Responsibilities

The theory ordinarily offered to account for the moral importance of these intuitively appealing special responsibilities deals in terms of self-assumed obligations. That analysis figures most famously in H.L.A. Hart's classic essay, "Are There Any Natural Rights?" which first introduced the concepts of special rights and duties into modern moral philosophy. "I think it is true of all special duties," Hart (1955, p. 185) wrote,

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[1] This is just updating and inverting Godwin's (1793, bk. 2, chap. 2) infamous example of being forced to choose between saving your mother or some greater public benefactor (Archibishop Fenelon) from a house fire. William's air crash example is related by Hare (1981, p. 138), who is highly critical of the conclusions reached by Williams there and elsewhere (Williams, 1981, pp. 17-18). See similarly Gorovitz (1977), Blum (1980), Oldenquist (1982), and Winkler (1982).

"that they arise from previous voluntary acts." Hart's own discussion is characteristically cagey, abounding with qualifications, exceptions, and ambiguities. His general principle, however, has passed into the philosophical conventional wisdom pretty well shorn of equivocation. Thomson (1971, p. 65), for example, asserts quite baldly, "Surely we do not have any . . . 'special responsibility' for a person unless we have assumed it, explicitly or implicitly" (see similarly Rawls, 1958, p. 158).

The great appeal of the model of self-assumed obligations is that it explains so neatly what is "special" about our special rights, duties, obligations, and responsibilities. Special obligations are distinguished from general ones in two ways. First, whereas everyone has the same general duties, special obligations vary from person to person: I have special responsibilities that you do not. Second, whereas general duties are owed to everyone equally, special obligations are owed to specific others: I have different responsibilities to different people. The analysis of special obligations as self-assumed obligations accounts for both features beautifully. According to that analysis, I have some obligations that others do not because I have assumed them and others have not; my obligations vary because I have assumed obligations with respect to some people but not others.

The paradigm case of a special obligation is, for Hart (1955, pp. 183-184) as for all advocates of this model, a promissory or contractual obligation. A promissor, through a voluntary act of will, imposes upon himself an obligation that is peculiarly his own, not shared by the world at large, and his obligation is owed to a specific individual, the promisee, rather than to the world at large. Various other special responsibilities—of businessmen to their customers and employees (Atiyah, 1979), of professionals to their clients (Fried, 1974, p. 78), of all of us to our families (Hart, 1958, p. 104) and friends (Montaigne, 1580/1958, p. 137)—are, within this model, seen as more-or-less attenuated instances of this same basic promissory pattern.

Perhaps the most important practical consequence of analyzing special responsibilities as self-assumed obligations is to reinforce our intuitions about their restricted scope. After all, most of us voluntarily assume responsibility in any way whatsoever for only a very limited number of people. On this account, we have no special responsibilities (but only, at most, much weaker general responsibilities) that have not been voluntarily self-assumed.

The model I want to counterpose to that traditional one traces our special responsibilities to the peculiar vulnerabilities of specific others to our actions and choices. It is their vulnerability, not our promises or any other voluntary act of will on our part, that imposes upon us special responsibilities with respect to them. The promissory obligations that figure so centrally in the traditional account are, on this analysis, just a special case of vulnerability. If I promised and others are depending on me in consequence, then I am obliged to do as I promised—not because I promised, but merely because they are depending on (i.e., are vulnerable to) me.[2]

Whereas the paradigm of self-assumed special obligations is the promise or contract, the paradigm for the vulnerability model is family responsibilities. Hart (1955, p. 187) himself admits the difficulty of assimilating these rights and duties to his model of self-assumed obligations. And well he should. You do not enter into families voluntarily. You do not ask to be born; you do not choose your parents or siblings. In the first instance, anyway, you make them no promises and extract none from them.

Even areas of family life that appear on first brush to be contractual turn out not to be completely so. The standard "marriage contract," for example, deviates crucially from the legal ideal of a contract. The authors of the *Second Restatement of the Law of Contracts* are at pains to emphasize that fact: "Although marriage is sometimes loosely referred to as a 'contract,' the marital relationship has not been regarded by the common law as contractual in the usual sense" because "many terms of the relationship are seen as largely fixed by the state and beyond the power of the parties to modify" (Braucher & Farnsworth, 1981, sec. 90; see further Pateman, 1981). Even if marriage partners were to write their own contract, there would be certain duties (such as the duty of support) that would be regarded legally as "essential features" of the marital relationship and which the two parties involved would be legally unable to alter, even by mutual consent. That, of course, violates a fundamental precept of contracting.[3]

To see how completely ill-suited a model built around contractual or quasi-contractual notions of reciprocity is to the analysis of family responsibilities, consider just two of its more absurd conclusions. First, on that account, a child who lived at home until age 21 would have incurred a debt to his parents 16.6% greater than that of his sister, who left home at 18. But surely the children's obligations to aid their ill and aged mother depend merely on her present needs and their relative capacities to meet them, and not on their past performances. It would be absurd to suggest that the son who remained at home longer should pay 16% more, even though he is now earning 50% less than his sister. (If it really were a case of debt, notice, it would not be absurd: our business obligations are invariant with respect to our capacities to meet them, at least until we declare bankruptcy.) Second, it would be equally absurd to suggest that the debt children owe their parents might at some point be fully repaid, and the children relieved of any further obligation to help parents, however ill they may be. Once contractual debts have been met, any special relationship between creditor and debtor dissolves. The two parties then stand in exactly the same relationship as before the debt was incurred. That is simply not how families or friendships work. A favor and a return-favor do not cancel each other out and dissolve the relationship; instead, they strengthen it (English, 1979, p. 345; Heath, 1976, pp. 59-60).[4]

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[2] Hence, I basically have no obligation to keep a promise when the promisee is not depending on me in any way to do so. The only reason I then would is that everyone in society depends on the maintenance of the institution of promising, which is vulnerable to being undermined (however slightly) by my breach. That fact explains why the courts award damages, but only nominal ones, in cases where one party suffers no loss from the other's breach of contract. See Braucher and Farnsworth (1981, sec. 346) and, more generally, Atiyah (1979), Gilmore (1974), and Kronman (1981).

[3] Champions of the voluntaristic model might reply that the parties have at least voluntarily assumed their roles and the concomitant (albeit unalterable) duties.

What most fundamentally underlies the reciprocal duties of family life—of spouses to one another, of parents to their children, of children to their aged parents—is the vulnerability of those parties to one another. The protracted dependency of the human infant is, biologists tell us, one of the most striking features of the species. Much vulnerability is material in nature. Wageless members of the family depend on the wage-earner(s) to bring home the bacon; children depend upon their parents to feed, clothe, and shelter them; in the absence of adequate collective provision, ill or infirm parents are often dependent upon their children for care of a very material sort.[5] But we must not focus exclusively upon these material forms of vulnerability. Much of the vulnerability, in family life and friendships more generally, is of an emotional sort. That fact provides a crucial link in the argument explaining why the material support in question has to come from the particular person it does (for example, the parent, child), instead of some unrelated other who might be in an objectively better position to provide it.[6]

One way to demonstrate the superiority of the vulnerability model would be to survey systematically those "special relationships" that we so readily acknowledge. Among the cases that might be discussed, in addition to the family responsibilities just mentioned, are the special responsibilities of businessmen for the safety of their employees and customers, of professionals (especially doctors and lawyers) for their clients, of friends to one another, and of beneficiaries to their benefactors. The aim would be to show that the vulnerability model can offer a plausible account of all features of all these special responsibilities, whereas the model of self-assumed obligations can at best account for only a few features of a few of them. Such a case-by-case demonstration is, however, beyond the bounds of the present essay and is conducted elsewhere (Goodin, 1985a, chap. 4) instead.

The best quick demonstration of the superiority of the vulnerability model is this. Imagine someone who is utterly helpless. The model of self-assumed

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Although that may explain why marriage partners have special responsibilities, no such voluntaristic analysis can explain what they are; for that, a separate account must be given. The vulnerability model, in contrast, provides a unified account of both the fact and the content of those duties.

[4] Where families and friendships are concerned, reciprocal favors create bonds transcending the simple sort of trust that underlies long-standing commercial relationships, which amounts to little more than "trust to honor your debts and to keep your word" (Hardin, 1982; Macneil, 1980).

[5] I treat these as "social facts" which should, at least provisionally, be taken as given. In these and many other cases, people end up being vulnerable to certain particular individuals merely because society happens to assign those particular people responsibility for caring for them (for example, parents for children). The only reason that this social allocation of responsibility matters morally is, I would argue, because of the vulnerability and dependency that it engenders. If it is regarded as your job, and no one else will do it if you do not (and if furthermore it is a job that really ought be done by someone), then there is a strong argument for you to do it. That is not to say that it should be your job, or that you (or we) should not try to get someone else to accept responsibility for doing it. It is merely to say that until and unless they do, you should keep on performing it. This is a topic to which I return in discussing collective responsibilities.

[6] Thus, although the courts are committed to doing whatever is in "the best interests of the child," children are not removed from marginally less suitable natural parents whenever marginally more suitable foster parents can be found, and the reason given (the risk of "separation trauma") points, just as my argument here suggests it should, to the emotional vulnerabilities of the child. See Mnookin (1973) and, more generally, Parfit (1984, p. 96) and Blum (1980, p. 56).

obligations says you have only such responsibilities with respect to that person as you voluntarily assume. You might agree to do all sorts of nice things for that person. It would be terribly kind of you to do so, and of course once you have assumed responsibilities it would be obligatory at that point for you to discharge them. But, within this model, you are initially under absolutely no obligations to agree to do all (or any) of those favors. You are morally at liberty to press your bargaining advantage to the hilt, and to exploit mercilessly the other's weakness. You can, and morally you *may,* force that person to agree to arrangements that are highly inequitable, to say the least. For examples, if any are needed, reflect upon the classic cases of "unconscionable contracts": peddling useless drugs to desperately ill patients at inflated prices, or charging $1000 for a hamburger when you find yourself enjoying a monopoly on food at some disaster site.

Now, according to the model of self-assumed obligations, parties in strong bargaining positions like these would be violating none of their moral responsibilities by pressing their advantage to the hilt. The vulnerability model, in contrast, regards it as the height of immorality for them to exploit the other's weakness in this way. The same thing that would enable the stronger to drive a hard bargain with (and, within the model of self-assumed obligations, to evade altogether any responsibility for) the weaker would, within the vulnerability model, impose a heavy responsibility upon the stronger to look after the weaker. In such cases, the implications of the two models diverge clearly. And surely our considered moral judgments, backed up by all our "background theories," side with the vulnerability model on this score. As Green (1881, p. 372) remarked, "There is no clearer ordinance of that supreme reason, often dark to us, which governs the course of man's affairs, than that no body of men should . . . be able to strengthen itself at the cost of the other's weakness."

### Protecting the Vulnerable

In spelling out the details of this vulnerability model of special responsibilities, I shall confine myself to three general remarks. The first concerns definitions of key terms: vulnerability, dependency, and responsibility. I use "vulnerability" and "dependency" interchangeably to refer to the following situation: *A* is vulnerable to *B* if and only if *B*'s actions and choices have a great impact on *A*'s interests. Here I equate "interests" with "welfare," following Barry (1965, p. 176; see also Reeve & Ware, 1983) in reading "*x* is in *A*'s interests" to mean "*x* increases *A*'s opportunities to get what *A* wants." Protecting the vulnerable is thus morally desirable because, *ceteris paribus,* it is morally desirable that people's interests and welfare should be furthered.[7]

Central to the concepts of "vulnerability" and "dependency" is the fact that they are relational. You are always vulnerable *to* or dependent *upon* some individual or group of individuals who have it within their power to help or to harm you in some respect(s). This relational character of the concepts is an enormous help in deciding where to settle responsibility for rendering assistance. If vulnerability gives rise to moral claims, then those moral claims must be principally against those agents to whose actions and choices one is vulnerable. Thus, this might be characterized as a "directed needs" model. Saying merely that "*A* is in need" leaves unspecified who should be responsible for meeting those needs. Saying that "*A* is vulnerable to *B*" provides a ready answer to that question.

Vulnerabilities are also relative. *A* would be more vulnerable to *B* than to *C* if *B*'s actions and choices make a greater impact on *A*'s interests than do *C*'s actions and choices. Those to whom one is relatively more vulnerable have relatively greater responsibilities. Anyone to whom *A* is uniquely vulnerable (no one else will help if that person does not) has the greatest responsibilities of all.

Although primary responsibility falls to whomsoever is in the best (or, in the limiting case, the unique) position to protect the vulnerable, that does not relieve others of responsibility altogether. Those who could help, albeit not as well as those with primary responsibility, retain a residual responsibility to do so should the others default, and they also have a continuing responsibility to monitor the situation to see whether or not their assistance is in fact required (Feinberg, 1970, p. 244).[8]

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[7] My principle is more catholic than it might appear. Although the definition of "interests" is recognizably liberal, the slippage between what people want and what is objectively best suited to satisfying their desires allows substantial scope for Marxian notions of false consciousness to operate. Similarly, although my principle is welfare-consequentialistic in form, deontologists can embrace it just so long as the *ceteris paribus* clause is understood to include their cherished rights-based constraints on action. Notice also that my principle treats acts and omissions symmetrically. Glover (1977, p. 94), Goodin (1982b, pp. 14-15), and Bennett (1983) show that, once other morally relevant correlates are factored out, not helping when you could have done so is morally indistinguishable from causing harm when you could have avoided doing so. That argument applies with particular force to bearers of standard special responsibilities (e.g., bodyguards and firemen), and would by extension apply equally forcefully to all vulnerability-based responsibilities that I have here argued are analogous.

Let us consider next the definition of responsibility. There are various forms, among them: causal responsibility (you produced this result); moral responsibility (you are to blame for it); and task responsibility (it's your job). Often we slide carelessly from one sense to another. To employ a pun that has frequently been mistaken for an argument, it is commonly said that those who are responsible should be made responsible. That is to say, (task) responsibility should be settled on those who are (causally) responsible. If *A*'s actions have caused some unfortunate situation, then according to this rule it should be *A*'s job to correct it. Most especially, if *A* got himself into a jam, it should be *A*'s responsibility to get himself out of it.

My argument is that task responsibility and causal responsibility are separable, logically, and should often be separated in practice. On my analysis, how *A* got into his present state is irrelevant. All that matters now is who is best able to get him out.[9] Sometimes—maybe even typically, for all I know—the person causally responsible for producing a state of affairs will be best able to reverse it. If so, then the pun will point us in the right direction. But in a great many cases, the agent best able to get a person out of a jam might well be someone other than the agent who got that person into it. Then, I would argue, task responsibility should be settled upon the former, even though that agent is in no way causally responsible. When the wake from a passing speedboat capsizes my sailboat, it must be the responsibility of other nearby sailors rather than the long-gone speeder to pull me out of the water.

That rule might, admittedly, tend to encourage improvident behavior. People could always rely on others to clean up their messes or get them out of jams they got themselves into. But providing appropriate counters to those perverse incentives is a practical rather than a moral matter. Incentives, as Rawls (1971, sec. 48) and Feinberg (1970, p. 94) rightly emphasize, are nothing more than socially necessary bribes; they do not necessarily have anything at all to do with moral deserts. On this nonmoralized view of incentives, encouraging people to get others out of bad situations is, in principle, just as important as discouraging those people from getting themselves into those situations in the first place. Where the emphasis should fall, in any particular instance, is simply a technical optimization exercise: we should just choose the most cost-effective method, or combination of methods, for avoiding suffering all around. This typically leads us to assign responsibility largely (and, on some analyses, almost exclusively) to people who were in no way causally responsible for the situations they are now being asked to remedy.[10] Furthermore, there are in any case clear limits on the use of incentives. They must be effective in accomplishing their desired goal; they must be no larger than strictly necessary to do so; and they must be no more costly than the goal itself is worth. All this combines to suggest that incentive considerations will not lead us to deviate often or largely from the course of action recommended by my principle of protecting the vulnerable.

The separate question of moral deserts remains.[11] It presents itself as a counterargument to my vulnerability model in two respects. One argument concerns those who find themselves in difficulty through their own improvidence: some would say that they had it coming, that is, they deserve whatever they got and it would actually be *wrong* for anyone to help them out of their difficulty. The other argument concerns those who have done nothing to cause another's distress but are (perhaps uniquely) in a position to help relieve it: some would insist that they have done nothing to deserve this burden, and that it would be wrong to impose it on them although of course it would be terribly nice of them if they chose to assume it.[12]

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[8] Notice an important asymmetry. When those with primary responsibility have acted, those with subsidiary responsibilities are relieved of any obligation to do so. But when those with subsidiary responsibility have, owing to the inability or especially the unwillingness of those with primary responsibility, been forced to act in their stead, that in no way relieves the latter of their obligations or excuses their defaulting.

[9] This saves my principle from the counterintuitive consequence that we have stronger responsibilities for protecting the rich than the poor, because, *ceteris paribus,* people are more vulnerable, the more they have to lose. But of course the more you have to lose, the better able you are to protect yourself from loss: the rich can protect themselves; it is the poor who need our help.

[10] The incentive problem posed here is discussed more fully by economists concerned with allocating the costs of accidents between those whose negligence created a dangerous situation, on the one hand, and those with the "last clear chance" to avoid the accident, on the other. Wittman (1981) has shown that the optimal incentives there require us to impose the bulk of the charges on the latter party rather than the former one. This is consistent with my suggestion that task responsibility should be predominantly forward-looking (who can remedy the situation?) rather than backward-looking (who caused the situation?).

[11] As a kind of hybrid between the incentive and desert arguments, there is a third proposition, which holds that it is our moral responsibility to protect the long-term interests of the profligate poor (or their or our successors, who would otherwise have to pay their bills) by creating appropriate incentives in the present to encourage them to do what they (or their or our successors) will at some future time wish they had done. But the same limits apply to this argument as the earlier incentive argument: it justifies incentives only insofar as they are necessary, effective, and entail less overall harm than benefit to the interests they are designed to protect. Thus, if the arguments of footnote 19 below are correct, the issue will simply not arise. Even if it does, this duty to be "cruel to be kind in the long term" might be counterbalanced by our duty not to be cruel in the short term.

Both claims, however, extend the notion of "deserts" beyond its proper bounds. Take the second counterargument first. Incurring a duty or a responsibility is not necessarily, nor even characteristically, something that you deserve. More often than not, it is something that just happens to you. When walking along a deserted beach, you see a child floundering in shallow water. Nothing in your background or character suggests that you deserve to bear the burden of effecting the rescue. It would, however, be absurd to say that you are therefore morally at liberty to leave the child to drown.

The notion of "deserts" is ordinarily equally out of place in the first counterargument as well. By and large, the improvident should be regarded as foolhardy people whose recklessness is to be discouraged through appropriate disincentives, not as evil people whose wickedness deserves to be punished. In dealing with people in precarious situations, questions of fault, blame, and desert are simply out of place. When two victims of a traffic accident are brought into the hospital emergency room with similar injuries, surely they should be treated with equal care and attention, even though one caused the accident while the other was its innocent victim. The need, dependency, or vulnerability of the victims, not their deserts, is what dictates physicians' responsibilities (Goodin, in press).

The third and final general point about my vulnerability model is that the "agents" to whom these responsibilities fall might be either individuals or collectivities. Consider the problem of famine relief. Millions of people are starving daily. But realistically what could I do to help? There are millions of them and only one of me. In such cases it may make little sense to hold anyone individually responsible; maybe, in my terms, those in need are just not very vulnerable to any single other individual.

Such invulnerability would, on my argument, relieve us individually of any responsibility for helping with famine relief efforts. Excuses of that sort, however, typically serve merely to get us out of the fire and into the frying pan (Austin, 1956). The price of letting individuals off the hook in that way is to put collectivities there in their place. The starving people of the world certainly *are* vulnerable to the actions and choices of those of us in rich nations, taken collectively. Hence we collectively have heavy responsibilities in respect of them.

Collective responsibilities are laid, in the first instance, upon the group as a whole to organize and to implement a scheme of coordinated action to protect the vulnerable agent or agents' interests. But these group responsibilities ultimately devolve onto individuals constituting the group. Each member of the group is responsible, so far as that person is able consistently with that person's other moral responsibilities: 1) to see to it that the group organizes a scheme of coordinated action that protects the vulnerable agent or agents' interests as well as it can; and 2) to discharge fully and effectively the responsibilities allocated to him or her under any such scheme that might be organized, just so long as it protects the vulnerable agent or agents' interests better than no scheme at all would protect them.[13]

In many ways, making something a collective responsibility only complicates things. In one important respect, however, it simplifies them. Making something a collective rather than an individual responsibility removes any objection to the social enforcement of that responsibility. Individuals have many moral duties which, for one reason or another, we think society ought not compel them to discharge. Where the duty is a collective one from the outset, however, collective action is clearly appropriate. Then the collectivity is fully justified in compelling each of its constituent parts to play its full, appointed role in discharging those obligations, duties, and responsibilities.

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[12] The most famous counterexample of this sort is Thomson's (1971, p. 48) of a dying violinist, who can survive only if plugged into your circulatory system. My principle does indeed require you to give the violinist's interests serious consideration; but they may of course be outweighed by the other responsibilities, including responsibilities to yourself, on which you would have to default to save the violinist's life. In this way my principle can wriggle out of Thomson's counterexample. Here it is to the credit of my theory that it requires some squirming: they simply answer, "It's my body, and that's that," surely makes far too short work of what truly is a terrible moral dilemma.

[13] Philosophers (McKinsey, 1981) tend to recognize the latter duty but not the former; for more details see Goodin (1985a, chap. 5). There is obviously some scope for trading off these two duties for one another, with political action designed to alter the scheme being preferred to cooperation under the scheme whenever it is likely to be sufficiently effective in instituting a scheme that is sufficiently better at promoting the vulnerable agent or agents' interests sufficiently quickly to justify the short-term damage to those interests.

## Broader Implications

The aim of this essay is to demonstrate that we have a broader range of social responsibilities than we traditionally acknowledge. What I hope to establish here is essentially a point of principle. The practicalities—how best to discharge these broader responsibilities—largely remain as open questions, although at the end of this essay I shall offer reasons for believing that the welfare state is a good way of discharging at least some of them.

The argument for broader responsibilities goes like this. We all acknowledge strong responsibilities toward our own families, friends, and certain others. Once we start examining the sources of those responsibilities, we discover there is nothing "special" about them. It is the vulnerability of the others, rather than any voluntary act of will on our part, that generates those responsibilities. There are many more people vulnerable to us, individually or especially collectively, than stand in any of the standard "special relationships" to us. If my analysis of the true basis of those standard responsibilities is correct, then we have strictly analogous (and, potentially, equally strong) responsibilities toward all those others as well. Aid to vulnerable strangers is thereby justified on the same basis as aid rendered to our own parents or children.

To see what all is at stake in this argument, its ramifications must be sketched, however briefly and unsatisfactorily. First and foremost, notice that state welfare services are essentially mechanisms whereby we attempt collectively to protect vulnerable members of our own society. They may or may not be the best mechanisms available for so doing, and I shall return to that issue presently. But for now, just note that the welfare state, as a device for protecting the vulnerable, can claim at least *prima facie* moral justifiability on my principle.

The vulnerability model, in its collective form, might also underwrite far stronger international obligations than are ordinarily acknowledged (Singer, 1972). People in very poor countries are enormously vulnerable to the actions and choices of people in very rich ones. Again, how we might best go about discharging these responsibilities is an open question and turns heavily upon various matters of empirical fact. It seems very likely, however, that the responsibility imposed on the rich will turn out to be a collective rather than an individual one.[14]

Yet another potential application concerns our obligations to future generations. Intergenerational transfers, especially spanning several generations, are singularly one-way affairs. We can do whatever we like to our distant successors without fear of retaliation. Future generations are enormously vulnerable to the actions and choices of present people, both individually and especially collectively. The things we destroy or use up will be unavailable for them to enjoy. Their extraordinary vulnerability to our actions and choices seems to be the best basis for assigning strong responsibilities to present generations to provide for the further future.

Finally, this model might entail responsibilities to protect animals and natural environments. "Dumb animals," delicate coral reefs, and ecosystems more generally have proven enormously vulnerable to the actions and choices of human agents, both individually and especially collectively. Whether or not that gives rise to any responsibilities on our part to protect them depends on whether this constitutes "vulnerability" in the relevant sense. Earlier, "vulnerability" was defined in terms of the impact of our actions and choices on the *interests* of those who are affected. The question thus becomes one of whether animals and other nonhuman entities can have genuine interests. This is not the place to enter into that debate.[15] It is enough to say that if (and insofar as) we see sentient creatures, ecosystems, or the natural environments upon which they depend as having "interests" at all, then their vulnerability will once again generate *prime facie* responsibilities on our part to protect them.

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[14] Glover (1975) points out that, although none of us acting alone could eliminate world poverty, virtually any of us acting entirely alone could lift at least several starving Asians out of poverty. The essential flaw with individualistic programs such as "adopt-a-child," though, is that they work on a child-by-child basis; consequently they can end up doing as much harm as good, both for the putative beneficiaries and for those around them. To be sure our well-intentioned gifts have the desired effects, they must be set in the context of some reasonably comprehensive and well-integrated scheme for restructuring the recipient community as a whole. That requires a kind of coordination well beyond anything isolated donors—or even relatively large-scale voluntary charities—could reasonably hope to accomplish. Nozick (1974, pp. 266-267) makes just such a case for organized collective action.

[15] Feinberg (1980, pp. 159-206) persuasively argues that animals, at least, do have interests and hence interest-based claims; and since Routley and Routley (1979) are right to insist that the burden of proof must fall upon anyone who proposes to treat different agents' interests differently, animals' interests must at least presumptively enjoy the same status as our own.

Of course, we will have to balance off one set of responsibilities against others. How much should we allocate to our collective responsibilities for social welfare, as against our private responsibilities for the well-being of our families and friends? How much to foreign assistance, as against domestic social services? How much to future generations, as against present people? How much to the protection of animals and environments, as against humans?

Some might regard these responsibilities as utterly incommensurable and therefore deny that any such balancing can ever be done. Recall Sartre's example of "the young man who is torn between remaining with his ailing mother and going off to join the Resistance. Sartre's point is that there is no way of adjudicating between these two strong claims on his moral allegiance through reason. . . ." But, as Taylor (1976, pp. 290-291) goes on to say, if there really were no bases for comparing the claims of kin and country, then the moral dilemma itself would disappear: the son should just "throw himself" one way or the other, as Buridan's ass in choosing between two equally good haystacks; since neither choice would be right or wrong, there would be no point agonizing over the choice. But that cannot be a correct analysis. We may find it hard to say which is the right choice, but we are quite confident that, whatever else, it is rightly to be regarded as a hard choice. That fact is conclusive evidence that the responsibilities cannot really be incommensurable, after all.

Here I suggest that the notion of "vulnerability," cashed out in terms of the notion of "interests," can not only account for the force of these dilemmas but can also help us resolve them. The dilemmas arise because discharging our responsibilities with respect to some of those who are vulnerable to us entails defaulting on our responsibilities with respect to some others who are also similarly vulnerable to us. Which we should favor depends, I suggest, upon the relative vulnerability of each agent. We must determine 1) how strongly that agent's interests would be affected by our alternative actions and choices, and 2) whether or not that agent would be able to find other sources of assistance or protection should we fail to provide it.[16]

On balance, it seems likely that those relatively near to us in space and in time probably will be rather more vulnerable to us in these ways. Hence my argument is saved from the traditional *reductio* of requiring us to give everything we have to starving Asians, or always to be saving everything for the ever-receding infinity of future generations, or to have our own lives and projects constantly interrupted to serve others. My argument would seem to allow—indeed, to require—us to show *some* bias toward our own kind, however defined. But that bias must not be absolute. The vulnerability of others—be they needy fellow-citizens, foreigners, heirs or hares—to our actions and choices may well be sufficiently large, and how we (and perhaps we alone) can help sufficiently clear, to require us to give their interests some substantial weight in reckoning our own responsibilities. Charity may indeed begin at home, but morally it must not stop there.

## Reducing Immoral Vulnerabilities

Up to this point, my argument has taken vulnerabilities and dependencies as given. I have argued that, if people are vulnerable to us, then it is our responsibility to protect them. Sometimes, however, it might be even better to try to eliminate the conditions making them vulnerable and dependent. A few of these conditions may be naturally fixed, inevitable and immutable. But most vulnerabilities will turn out to have been created, shaped, or sustained at least in part through certain social arrangements (Titmuss, 1958). In some respects, this is true of even those dependencies that would seem at first brush to be the most natural, such as those connected with childhood (Marks, 1975; Skolnick, 1975) or old age (Walker, 1980; 1982).

It is obviously immoral to hold people in an unnecessarily dependent status against their will. Moral theories built around notions of free choice, self-respect, or autonomy all yield that same conclusion. But in some cases people willingly do—and it seems morally and prudentially desirable that they should—render themselves vulnerable to a variety of other people for a variety of purposes. In personal relationships, love can plausibly be analyzed as putting yourself in one another's power. "A world in which no one had power over another, and in which no one was vulnerable, a world in which people could be moved to action by force or reason alone, would be a world without love, an inhuman world"

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[16] We also need to decide how to weight one agent's interests relative to another's. Where human beings are concerned, I am firmly inclined to weight everyone's interests equally (Goodin, 1975). I am just as firmly inclined to weight future generations' interests the same as our own (Goodin, 1982a). But where animals or inanimate objects are concerned, I am far less certain what the appropriate weighting should be.

(Wilson, 1978, p. 315). In commercial relations, interdependence is the essence of trade and hence of all mutually profitable business ventures. Similarly in politics, interdependence forms the basis of all kinds of alliances and coalitions (Baldwin, 1980). Objectionable though we might find the particulars of any specific arrangement, there is surely nothing wrong with exchanges or alliances as such merely because they engender vulnerabilities of this sort.

Our moral theories must, then, allow for vulnerabilities, some of which are inevitable and some of which are self-selected. Yet at the same time we must recognize that such relationships are subject to abuse, and we must insist upon certain conditions to guard against that kind of abuse. The standard proposal is that, insofar as vulnerabilities or dependencies are within our power to create or alter or eliminate, they should be reciprocal or mutual. The real objection, various commentators observe, is not to dependency as such. It is instead to *unilateral* dependency, to *asymmetrical* power relations. That asymmetry is what creates opportunities for the strong to exploit the weak. Were each party equally dependent upon the other, there need be no fear of such behavior (Wilson, 1978, p. 315; see similarly Held, 1974; Pateman, 1981; Thompson, 1825; and Wollstonecraft, 1792).[17]

It is, however, wildly unrealistic to demand that dependencies or vulnerabilities be completely symmetrical, that is, that each depend on the other in exactly the same measure. That condition is satisfied in virtually no trading relation, in virtually no alliance, and probably in very few personal relationships. Neither is complete symmetry strictly necessary for a morally acceptable dependency relationship. There are, in fact, two acceptable alternatives.

The first is described by a pair of conditions. One is interdependency: each party must get something out of the relationship; or, alternatively, each must stand to lose something if it were terminated. The other condition is that neither party is so heavily dependent upon the other that the dependent party would cease to meet basic needs were the relationship terminated (Baldwin, 1980; Shue, 1980). The latter condition could be waived in cases of perfectly symmetrical vulnerabilities but becomes increasingly important the greater the asymmetry.[18]

The second alternative would permit monopoly suppliers of needed resources to exist. What it would crucially demand is merely that they have no discretionary control over the disposition of those resources. *B*, who needs the resource, would still be dependent upon *A*, the monopoly supplier of that resource. But *A* cannot *exploit B*'s dependency unless *A* can withhold the resource at will. And it is exploitable dependencies which we find morally most offensive.

## An Application: The Welfare State

The *raison d'etre* of the welfare state clearly is to discharge in part the responsibility I have here been discussing, namely, to protect vulnerable members of society. The early British initiatives—old-age pensions, health and unemployment insurance, minimum wage policies, and the like—were all aimed explicitly at "protecting the weakest and most vulnerable elements in society, such as the aged poor, the unemployed workman, the sweated worker" (Robson, 1976, p. 21). The subsequent history of the welfare state has similarly consisted in the increasing recognition of and response to "states of dependency" (Titmuss, 1958, pp. 42-43). Contemporary arguments for the expansion of the welfare state are all likewise couched in terms of dependency and vulnerability (van Lennep, 1981, p. 9; Australian Royal Commission on Human Relationships, 1977, vol. 1, p. 63).

Assuming my earlier arguments have proven persuasive, it is clear that we have a strong moral responsibility to protect those who are vulnerable to our actions and choices. The only remaining question is whether the welfare state is the best way of doing so. Of course, any particular welfare state may fail for reasons peculiar unto itself. It is important, therefore, to separate out and concentrate on what is generic to the welfare state as such.

For purposes of this discussion, I shall assume the defining features of the welfare state—and the features that must therefore be justified in any defense of it—are as follows. The welfare state necessarily entails *compulsory, collective provision for certain basic needs as a matter of right.* Welfare state provision is said to be compulsory and collective to distinguish it from private, voluntary charity; as distinct from more thoroughgoing egalitarian regimes, it provides only for people's basic needs; and as distinct from the old Poor Law, the welfare state allows beneficiaries to claim their entitlements as a matter of right.

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[17] "Symmetry" must be defined subjectively: what one party gains from the relationship (or would lose were it terminated) must occupy as important a role in that party's overall life plan as what the other party gains or would lose plays in that other's.

[18] Symmetrical dependencies, even very great ones, admit of no abuse. If each party depends equally upon the other for the satisfaction of basic needs, then neither could exploit the other through a credible threat to terminate the relationship.

The principal challenge facing any defense of the welfare state must surely be to show why protecting these vulnerable people should be a collective rather than an individual responsibility. There are basically two individualistic options available here. The first focuses upon the responsibilities of needy individuals themselves, while the second focuses upon the responsibilities of their families and friends.

Certainly it is true that people are even more vulnerable in even more ways to their own actions and choices than they are to those of the collectivity. For that reason, they should indeed bear primary responsibility for attending to their own needs. The state's responsibility is in the nature of a secondary, back-up responsibility, activated only when those with primary responsibility have failed to discharge it. Why they have failed, I have argued, is morally irrelevant: my analysis of responsibilities is, recall, forward-looking (asking only "Who is best able to help now?") rather than backward-looking (asking "Who is to blame for this situation?").[19] All that matters is that these individuals are now at risk of failing to have their most basic needs satisfied. That in itself is quite enough to activate secondary responsibility on the part of others to protect them.

The second individualistic option is to admit any need for someone to bear second responsibilities in such a case, but to lodge those responsibilities with some other private individuals rather than with the state. Specifically, this approach suggests that if people are unable to provide for themselves, it should be left to their families and friends to do so. That, curiously, is what "self-reliance" has come to mean in contemporary social policy debates (Goodin, 1985b). No doubt families and friends do, on my account, have strong moral responsibilities to provide assistance in such situations, and at its best, this system is no doubt preferable to state provision: material assistance from families and friends might betoken a deeper affection, in a way that state assistance necessarily cannot. The question is not whether families and friends should help out those in need. It is instead whether those in need should be forced to rely upon their families and friends for such support.

At this point, there is a strong case to be made for welfare-state provision as a kind of back-up to the back-up. An important part of this case is brutally empirical. One of the primary obstacles to an effective system of family responsibility for needy members is that needy people are ordinarily found in needy families, who are in no position to make any great sacrifices (Goodin, 1985b; Steiner, 1981, p. 114). There is also a principled objection to such policies, however, which would prove decisive even if these empirical facts were otherwise.

This principled objection harks back to the standards of an acceptable dependency relationship laid down above. Dependency might be acceptable if: 1) it were symmetrical; or 2) basic needs were not at stake; or 3) no one were to enjoy discretionary control over needed resources. *Ex hypothesi,* we are here discussing an asymmetrical relationship where the subordinate's basic needs are indeed at stake.

The only thing that could make such a relationship morally acceptable would be for the agent upon whom the subordinate depends not to have discretionary control over those needed resources. Where that agent is a person's family or friends, that condition cannot possibly be met: family and friends must inevitably enjoy discretionary control over resources that are, in the final analysis, still their own. Where the subordinate is dependent instead upon the state, however, it is possible to vest that person with a legal entitlement to assistance. Once such laws have been enacted, state officials (unlike families, friends, or private charities) lack any discretion in deciding whether or not to honor claims of needy petitioners (Goodin, 1985b; Smith, 1949, 1955; Titmuss, 1971).[20] The welfare state, defined as an institution which meets people's basic needs as a matter of right, is therefore a morally necessary adjunct to other more individualistic responses to the problems of vulnerability and dependency in our larger community.

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[19] Backward-looking considerations are relevant only insofar as, through incentive effects, they might have future consequences. But there is a substantial body of evidence suggesting that the poor tend to be poor planners and hence are largely impervious to incentives whether these are positive (e.g., incentives to slack off work when guaranteed a minimal income) or negative (e.g., incentives to avoid hazardous jobs); see Danziger, Haveman, and Plotnick (1981), Viscusi (1979), and Goodin (1985b). It is perfectly reasonable that the poor should be unresponsive to incentives: marginalist calculations are a costly business, and the poor have more pressing demands on their time and attention. Given that, however, restricting welfare benefits would probably amount to no more than bringing a gratuitous disincentive to bear on a group that is largely impervious to incentives of any kind.

[20] Discretion disappears only after such laws have been enacted; state officials are left with discretionary control over what laws they enact. But it is the exploitation of dependencies that worries us, and it is in the administration rather than in the enactment of welfare policies that the greatest danger (although certainly not the exclusive one) of such exploitation arises.

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# The Responsible Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974

ERIC M. USLANER
M. MARGARET CONWAY
*University of Maryland—College Park*

*Most analyses of the 1974 congressional elections have failed to find significant effects for either Watergate or personal financial conditions, despite the prominence of both of these issues in the campaign. An alternative thesis argues that the effect was indirect, through the selection of better-than-usual Democratic candidates and weaker Republican contestants for House seats. Reanalyzing campaign finance data, we challenge this thesis and then move on to a different type of analysis from that which traditionally has been done in retrospective voting studies. With the use of the 1972-1974 panel of the Center for Political Studies, we examine separately the voting behavior of what V. O. Key, Jr. called "standpatters" and "switchers." The former are motivated primarily by party identification, with small Watergate effects. Our probit analylsis for switchers, on the other hand, finds much weaker party identification effects, but, interestingly, much more pronounced Watergate and economic impacts. Furthermore, an analysis of the sample compared to the population of districts in 1974 suggests that a more representative sample would lead to even more pronounced impacts for Watergate and the economy than even we have found.*

Richard M. Nixon resigned the presidency on August 9, 1974 and was succeeded by the nation's first unelected chief executive, Gerald R. Ford, who then pardoned Nixon for his role in the Watergate scandal. After the oil price shock that accompanied the Yom Kippur War in the Middle East, inflation hovered close to 11% while unemployment rose to 5%. Americans were clearly up in arms over the state of the economy: 50.2% of the respondents to the 1974 Center for Political Studies election survey cited inflation as the most important national economic problem, a 2.5-fold increase over just two years earlier (Kiewiet, 1983, p. 84). At no time since these questions were first asked in 1958 had more than 20% of those surveyed been so concerned about inflation. Furthermore, 30.2% saw inflation as their most important personal economic problem in 1974, and an additional 7.1% referred to "declining real income." These two figures, when added together, represent a virtual doubling of such concerns from 1972 (cf. Kiewiet, 1983, p. 55). Indeed, the decline in real disposable per-capita income in 1974 (−2.3%) was the largest for any off-year since 1946 (Tufte, 1978, p. 25), when Democrats lost 56 House and 13 Senate seats and ceded control of the Congress to the Republicans.

It is hardly surprising, then, that the Democrats gained 49 House and four Senate seats in the off-year contests. Not since 1958, in the midst of a deep recession, had the president's party lost so many seats in the House; indeed, one has to go back to 1948, when the Democrats recaptured the Congress, to find greater Democratic gains than in 1974. The *New York Times* (1974) referred to the political terrain within its largest circulation area (New York, New Jersey, and Connecticut) as "wall-to-wall Democrats."

It would seem, then, that the 1974 midterm election would be easy to explain. Armed with the combined conventional wisdom of journalists and politicians, students of voting behavior also sought to demonstrate the effects of Watergate and the economy on the Republican debacle. But, with a single exception, these studies found either no effects or impacts so minimal that they were overwhelmed by traditional variables such as

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party identification. One study tried to put the intellectual carnage that the verious survey-based analyses had wrought in some sort of order by proposing a novel thesis: It was the candidates, not the voters, who reacted strategically to the Watergate and economy syndrome (Jacobson & Kernell, 1981). We examine the claims of the journalists and politicians and assess the findings of the studies of the 1974 election. After critically analyzing the "strategic politicians" thesis, we develop a reconceptualization of the retrospective voting argument and then show that there were, after all, Watergate and economic effects in 1974.[1] Our approach is to study not only the vote itself, but also vote changes from 1972 to 1974, using the Center for Political Studies panel survey. Our findings should be reassuring because they no longer lead to a schism between voting studies and conventional wisdom: 1974 was marked by retrospective voting.

## Watergate and the Economy

The conventional wisdom about the 1974 elections was summarized well by *Congressional Quarterly Weekly Report* (1974b, p. 3122) in its election post-mortem:

> Old-fashioned economic issues, centering around inflation and unemployment, emerged as the major topic of debate. Close behind was another warhorse of political rhetoric—corruption, focusing on the Watergate scandals, President Ford's pardon of former President Nixon and alleged violations of campaign contribution and spending laws.

The punishment of Republicans was selective: "Nearly every House Republican beaten . . . was counted among the conservatives; the liberal and moderate Republicans generally had little trouble winning reelection" (*Congressional Quarterly*, 1974a, p. 3065). Republican members of the House Judiciary Committee who voted against impeaching Nixon were significantly more likely to be defeated in 1974 than those who voted for impeachment, even controlling for the members' ideology (Wright, 1977). Republican Senator Charles Percy (IL), who was not up for reelection in 1974, and Democrat Wendell Ford (KY), who defeated Marlow Cook in that contest, both attributed GOP losses to the twin demons of Watergate and inflation. Republican National Committee chairman Mary Louise Smith also stated that "Watergate and the economy have combined to influence voters" (Cannon, 1974).

It thus appears that no great new theoretical insight is required to explain the outcome of the 1974 Congressional elections. Yet most of the attempts to assess the effects of Watergate and inflation have rung up "no sale." We are thus faced with a puzzle: How do we reconcile conventional wisdom with survey research?

Most systematic analyses of the 1974 midterm election, both those employing survey research and those using aggregate data, have produced no support for this scenario. Two analyses using the 1974 CPS surveys found virtually no Watergate effects and only small amounts of economic retrospective voting (Conway & Wyckoff, 1980; Miller & Glass, 1977). A Wisconsin study (McLeod *et al.*, 1977) also detected no Watergate effects. Kinder and Kiewiet (1979) found that personal economic grievances had little impact on vote choice in 1974, because most people attribute such complaints to their personal circumstances and not to governmental action. National business conditions, a form of collective judgment, also failed to yield significant coefficients. Only collective judgments of party competence, as Fiorina (1981) found, had a measurable impact on voters' decisions. Kiewiet's (1983, p. 105) more elaborate model of economic retrospective voting failed to yield any significant coefficients for either personal or national economic assessments for 1974; indeed, the coefficient for concern about inflation was precisely .00! Owens and Olson (1980), using aggregate data on vote shares for 1974 for 138 districts for which party registration figures could be obtained, found very little support for the supposed effects of either inflation or change in real per-capita income for the vote shares (or changes in vote shares) for incumbents of the president's party.

One might be tempted to respond that the most sophisticated study of retrospective voting in 1974 did find significant and large effects for both disapproval of the Nixon pardon and expectations about which party would best handle future economic problems in the congressional voting decisions for electors (Fiorina, 1981, pp. 164-167). Furthermore, attitudes toward the pardon clearly affected the voters' choice of party identifications in 1974, although alternative specifications yielded different conclusions about whether personal economic conditions or collective judgments on the state of the economy had a measurable impact on party identification choice that year (Fiorina, 1981, pp. 98-102).

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[1] To those who might doubt the roles of the economy and Watergate in the minds of the voters, it seems appropriate to recall the remarks of Rep. William Hungate (D, MO) of the Judiciary Committee during the impeachment hearings addressed to those who did not admit Nixon's implication in the scandal: "Some will say that it's not an elephant. It's a mouse with a glandular condition."

## Strategic Voters or Strategic Politicians?

One might simply choose up sides and prefer Fiorina's specification. This would hardly be unreasonable.[2] However, Jacobson and Kernell (1981) have offered a "revised theory" of the effects of Watergate and the economy. Their thesis is that when a party's electoral fortunes appear to be bleak, it will have difficulty attracting good candidates to run for office. On the other hand, in anticipation of an electoral sweep, the opposition will be able to attract challengers with previous elective office to run against the presidential party's incumbents. In turn, these candidates will be able to attract more campaign contributions. Because it is the challenger's spending that makes the difference in the outcomes of congressional races and not the incumbent's, the combination of a well-funded campaign and highly visible challengers makes the incumbent's task in winning reelection much more difficult. The chief advantage that House incumbents have over their Senate counterparts is that they are so much better known than their challengers (Hinckley, 1980; Mann & Wolfinger, 1980). Jacobson and Kernell (1981, p. 31) demonstrate that the average vote received by both Democratic and Republican challengers without prior elective office has been from 4 to 11% lower than those who have held office from 1972 to 1978. Furthermore (p. 32), the percentage of Democratic challengers with experience for both Republican-incumbent and open seats in 1974 was considerably greater than it was for any other recent election, whereas the share of GOP challengers with prior office was markedly lower in 1974. Additionally, Jacobson and Kernell argue:

> In the normal years, Democratic incumbents spent between 57 and 63 percent more than Democratic challengers; in 1974, they spent 22 percent less. Republican incumbents spent between 62 and 90 percent more than Republican challengers in the normal years, 293 percent more in 1974. . . . Campaign contributions—and therefore expenditures—were sharply responsive to perceived political trends in 1974. (1981, pp. 41-42)

A simple five-variable model including the party of the challenger, challenger expenditures, incumbent expenditures, the prior strength of the challenger's party in the district, and whether the challenger has held prior elective office accounts for 66% of the variance in aggregate vote shares in 320 congressional districts in 1974 (Jacobson & Kernell, 1981, p. 46). This thesis provides an alternative account of what went wrong for the GOP in 1974. It seems commonsensical, elegantly simple, and well supported.

However, a somewhat closer look at the evidence suggests that this explanation cannot fully account for the Republican debacle in 1974. First, the supposedly most important variable in the model, whether the challenger has held elective office, has the lowest standardized coefficient (.07) of the five variables. The variable with the highest *t*-ratio is whether the challenger is a Democrat. One does not need a very elaborate theory of voter responses to external conditions to predict that Democratic challengers would do well in 1974. Second, the campaign expenditure data can be given more than one interpretation. The comparisons made by Jacobson and Kernell and cited above are between Democratic incumbents and Democratic challengers, on the one hand, and GOP incumbents and hopefuls on the other. This assumes that only challengers' expenditures matter.[3] However, the regression presented by Jacobson and Kernell shows that challengers' vote shares are significantly related to both their own expenditures *and* those of the incumbents they run against. Furthermore, both Jacobson (1978, pp. 114-134) and Ragsdale and Cook (1984) demonstrate that incumbents react to challengers' fundraising by seeking more of their own resources. Whereas Jacobson argues that challengers simply raise all the money they can and it is incumbents who must react, Ragsdale and Cook provide evidence that challenger expenditures are strongly influenced by incumbent resources in the previous election. Even if the direction of causality were clearly as Jacobson posits, this still would not imply that incumbents' resources can be discounted completely in evaluating the strategic politicians thesis. Thus, we believe that a reasonable approach is to com-

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[2] There remains another possibility. Kramer (1983) presents an elegant argument that individual-level models of retrospective voting are misspecified and seeks to reconcile them with aggregate analyses. The latter studies consistently show much stronger retrospective voting patterns than do the microlevel ones, of which the 1974 results are but a special case. Kramer reviews many, but not all, of the conflicting findings. Much of Kramer's argument focuses on the difference between cross-sectional analysis of survey data and the time-series component of aggregate models. Our emphasis on vote change rather than the simple vote is consistent with Kramer's thesis, even if we do not use his elaborate model. (We are not aware of any attempt to apply his complex estimation procedure.)

[3] This is the argument implicit in Jacobson and Kernell (1981, pp. 41-42). Even though it is not explicitly stated, the context of the comparison was elucidated to us by Jacobson.

**Table 1. Ratios of Democratic to Republican Average Campaign Expenditures for the House of Representatives, 1972-1978**

| | Democratic Incumbent/ Republican Challenger | Open Seat | Republican Incumbent/ Democratic Challenger |
|---|---|---|---|
| 1972 | .603 | .514 | .366 |
| 1974 | .691 | .563 | .421 |
| 1976 | .588 | .596 | .331 |
| 1978 | .604 | .524 | .338 |

Source: Computed from Jacobson and Kernell (1981, p. 41).

pare spending levels of candidates who run against each other: Democratic (Republican) incumbents with Republican (Democratic) challengers. We have recalculated Table 4.1 in Jacobson and Kernell (1981, p. 41) and report the results in Table 1.

At first glance the figures appear to be supportive of the Jacobson-Kernell thesis. Spending ratios for Democrats in 1974 jumped substantially across the board. Indeed, Democratic challengers running against GOP incumbents finally appeared to be somewhat competitive with respect to financial resources. The largest percentage increase from 1972 to 1974 came among Democratic incumbents. The ratio of 1974 to 1972 advantages for that group is 1.15. The corresponding figures for open seats and for races with GOP incumbents are 1.10 and 1.09. The Jacobson-Kernell thesis would lead us to expect a sharp rise in funding primarily for Democratic challengers facing Republican incumbents, but in fact the increase was across the board, which suggests that Democratic races were viewed as good investments by contributors regardless of the incumbency status in a particular district.

Ratios calculated for 1976 versus 1974 indicate that the Democratic advantage for 1976 was of roughly similar magnitude for seats with Democratic and Republican incumbents (1.18 and 1.16). The Democrats actually did comparatively better in their funding of open-seat races in 1976 than they did in 1974. Finally, if the Jacobson-Kernell thesis were correct, we would expect that the ratio of Democratic to Republican funding would decline from 1976 to 1978, when the incumbent party was considerably less popular. This happened only for open seat races, as Democratic incumbents actually increased their advantage over GOP challengers while Republican incumbents had proportionately fewer resources (albeit by a miniscule amount) than they did in 1976.

If voters responded only to the quality of challengers, well-funded Republican incumbents and challengers with experience would have done as well in 1974 as they did in 1972, so that "Republican losses . . . would have been entirely self-inflicted. Presumably this would not happen very many times before politicians, who are not in the business of getting these things wrong, would discover that such strategies are badly flawed" (Kiewiet, 1983, p. 128). To corroborate the Jacobson and Kernell thesis, one would also need data on the prior background of state legislative candidates, because the Democratic sweep of 1974 was deep as well as wide. The Democrats took control of 15 additional state legislative chambers, losing only the Colorado Senate to the Republicans. One could also extend this argument all the way down the line—to city councils, even to sheriffs. Sooner or later, one has to reach a level of office for which there could be no conceivable prior elective position.

Yet, contrary to Kiewiet's argument, the alternative thesis that voters do respond to the economy (and to Watergate) is not quite so firmly established. Most of the evidence, including his own, is mixed. We suggest that the reason various studies have yielded conflicting assessments is because they are not asking the appropriate question. Studies of retrospective voting have almost all focused on cross-sectional analyses of why people vote the way they do. Did people vote Democratic in 1974 because they were concerned about either Watergate or the economy, on the one hand, or because they identified with the Democratic party, liked (disliked) Democratic (Republican) incumbents generally, or were closer to one party's candidate on the issues, on the other hand? These are interesting questions in their own right, but they do not address the issue of why the Democrats gained so many votes (and seats) from the Republicans in 1974 compared to 1972.

Put another way, why did so many people who either voted Republican for Congress or did not vote at all in 1972 vote Democratic two years later? The focus of retrospective voting studies has been the 1974 vote, but the central question should be the vote change. Consider voters who had cast a ballot for a Democratic congressional candidate in 1972. Clearly they were a numerous group since the Democrats took 52.7% of the two-party House vote that year. Furthermore, they were very likely to be Democratic party identifiers as well, as most Republicans and independents (particularly those who also voted for Nixon) supported GOP congressional candidates (Jacobson, 1983, p. 137). How could such a voter punish the Republican administration for the twin devils of Watergate and inflation? Only by voting Democratic again in 1974! But because most of these voters are Democrats anyway, it would hardly be surprising to find that party identification would swamp the effects of such retrospective factors, as it does even in Fiorina's analysis.

The appropriate way to examine the impact of Watergate and the economy is to consider voting changes from 1972 to 1974. Fortuitously, there is a panel conducted by the Center for Political Studies which can be used to examine just such questions. Key (1966) pioneered the study of vote changes, but his analyses depended upon people's recall of their previous ballot choices. Nevertheless, he found the electorate quite "rational" in presidential elections. Weatherford (1978) used the 1956-1958-1960 panel and found significant effects for vote changes from 1956 to 1958, controlling for party, at least for the working class.[4] Only Brody (1977) has analyzed the panel for comparisons between 1972 and 1974, but his emphasis was solely on change in party identification.

He and Fiorina maintain that there is considerable variation in partisanship even with the use of contemporaneous expressions of party identification at various waves of the panel. Indeed, 10 to 15% of respondents reported shifts in party identification from 1972 to 1974 and "party identification responds to short-term electoral forces" (Brody, 1977, p. 30). Specifically, he found that people who were dissatisfied with either Nixon (as measured by the feeling thermometer) or with their family income were more likely than the doubly satisfied to change party identification—and the doubly dissatisfied were three times as likely to change identification as the doubly satisfied. But Brody neither examines the impact of party-identification change on the vote nor presents a multivariate model, as does Fiorina (1981, chap. 5). We examine changes in the vote from 1972 to 1974 in the context of a more fully specified model. Whenever possible, our independent variables are not simply demographic or political constructs believed to affect the vote *direction.* Instead of examining voting change as a function of trust in government (or as a function of the Nixon feeling thermometer or voters' perceptions of the economy), we look at changes in trust in government, and the other relevant variables whenever possible. Some variables are simply not available in 1972 (the Nixon pardon question), whereas others do not need to be considered over time (party of incumbent in 1974). In general, however, to explain change, we believe that we must focus on change. We turn now to a specification of the models to be estimated.

## Data Analysis

The data base, as indicated, is the panel survey of the Center for Political Studies taken in 1972 and 1974. We use four dependent variables. The first is a measure of movement toward the Democrats in 1974, coded from 1 (people who either voted Democratic or for an independent minor party candidate or did not vote in 1972, and who voted Republican in 1974) to 4 (people who either voted Republican, voted for an independent or minor party candidate, or did not vote in 1972, and who voted for Democratic congressional candidates in 1974). The two intermediate categories encompass voters who cast GOP ballots or Democratic ones in both elections (cf. Weatherford, 1978). We also include a simpler measure for all voters in the panel: congressional voting behavior in 1974. This is the traditional variable used in retrospective voting studies, and it will thus provide a benchmark for comparisons with the change variables. We do, however, restrict the number of cases to the 467 for which we have complete data for the first dependent variable. The third dependent variable, encompassing switchers and new voters,[5] is derived from the first, taking only the two extreme values. The fourth, also derived from the first, includes only standpatters. The 229 cases in the sample residing in congressional districts in which there was no challenger are excluded.

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[4] The coefficient for the middle class was not significant. The coefficient for the full sample is not designated as significant in Table 3 (p. 930), but it has an *F*-ratio of 3.8, equal to a *t*-ratio of 1.95, marginally different from the conventional 1.96 used for the .05 level for a two-tailed test.

[5] Results only for switchers (excluding new voters) were virtually identical, so we opted for the slightly larger sample sizes obtained by including 1972 nonvoters. There was no theoretical reason to presume that the factors that mobilized 1972 nonvoters to select Democratic congressional candidates in 1974 were different from those that induced people who voted for the GOP in 1972 to switch.

Because regression analysis is not appropriate for categorical dependent variables, we use probit analysis (cf. Aldrich & Cnudde, 1975; Fiorina, 1981). However, in contrast to regression analysis, one cannot interpret probit coefficients in terms of amount of change in the dependent variable given a unit change in a predictor. Thus we must rely upon two alternative criteria. First, we can compare the *z*-scores for the predictors. Second, we use a technique developed by Wolfinger and Rosenstone (1981) which converts the estimated values of the dependent variable into probabilities for each case.[6]

The variables included in the analysis are discussed in the appendix. First, however, we discuss predictors that are not used. Although the literature on retrospective voting strongly suggests that collective economic judgments are far more important in individual voting decisions than is personal financial status (see especially Kiewiet, 1983), we were limited in the case of 1974 because only a post-election survey was conducted, and in 1972 the questions on government performance in managing the economy were asked of only half the sample. The results, however, indicate miniscule coefficients that fail to meet any reasonable significance level. None of the traditional demographic variables used in voting research (age, race, sex, occupation, income, education, marital status) reached acceptable levels of significance (at least .10). Finally, we wanted to examine changes in perceived change in financial conditions from 1972 to 1974 but were unable to do so because the overwhelming majority of respondents gave the same answer to the personal economic considerations in both years.[7]

The variables selected for analysis include: 1) the traditional party identification question, measured in 1974, and encompassing only the three-point scale recommended by Brody (1977) and Fiorina (1981); 2) change in strength of party identification from 1972 to 1974; 3) change in trust in government from 1972 to 1974; 4) change in one's placement of Nixon on the feeling thermometer from 1972 to 1974; 5) expenditures for 1974 by Republican challengers; 6) whether the incumbent in the congressional district was a Democrat (cf. Fiorina, 1981); and 7) a dummy variable scored at 1 if a respondent either perceived his or her financial situation to be worse in 1974 or who disapproved of the Nixon pardon *or* both and at 0 only for people whose personal finances were not perceived as worse and who also approved of Ford's pardon of Nixon. Neither condition alone produced very many changes in partisan affiliation, and our initial work confirmed that there were only modest effects for either variable on vote changes. We thus followed Brody's lead and hypothesized that the relationship was more complex. Brody (1977) noted that changes in party identification were enhanced among people who both disapproved of the pardon and who had financial grievances. This variable should have positive coefficients. We expect negative signs for the two party-identification variables. Initially, negative coefficients were anticipated for the change in trust variable, because the events of Watergate might lead to a decline in trust in government. A similar direction is hypothesized for the change in the Nixon feeling thermometer and for GOP challenger expenditures. Finally, a positive sign was hypothesized for the variable "incumbent Democrat." Most straightforwardly, people who were already represented by a Democrat in 1974 would boost that member's vote share. Curiously, as would be predicted by the Jacobson-Kernell model, there was no significant effect for a similar variable for the Republicans *in any direction*.[8]

We are particularly interested in comparing the behavior of switchers and standpatters, as Key (1966) did. The equations for all voters is of some interest since it helps to uncover similarities be-

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[6] Operationally, we take the estimated probit equation and derive predicted values for the dependent variable for each case, setting the variable(s) of particular interest first at the minimum value(s) observed. Then we use a routine provided by Steven Rosenstone to convert these predicted values (or *z*-scores) into probabilities for each case. The second step repeated the first except that the variable(s) of interest are set at the maximum observed value(s). The two probabilities are aggregated, and means are computed. The difference between the means for the maximum values and the minimum values indicates the net impact of the variable in terms of the probability of the direction of the vote (change).

[7] Similarly, we attempted to create two variables for the change in Nixon feeling thermometers from 1972 to 1974 to control for those whose earlier placements were above and below 50 respectively. However, the number of zero values was again extremely large.

[8] Unlike some of the other variables, incumbent Republican was not so strongly correlated with other variables that it should have been deleted on that basis alone. Our criterion for multicollinearity is that suggested by Farrar and Glauber (1967): the multiple correlation for any predictor with the remaining independent variables must be no higher than the multiple correlation between the dependent variable and the entire set of independent variables. On the other hand, money spent by Democratic challengers was highly correlated with the variable "incumbent Democrat." Yet it failed to be significant, even in models without the latter predictor.

**Table 2. Probit Analyses for 1974 Voters on Congressional Elections**

| Predictor | All Voters (Four Category Rank Order) | All Voters (Dichotomy) |
|---|---|---|
| Party ID | −.409***<br>(−7.041) | −.857***<br>(−11.118) |
| Party ID change | −.042<br>(−.687) | .050<br>(.579) |
| Change in trust | .042<br>(1.265) | .057<br>(1.207) |
| Change in Nixon feeling thermometer | −.002<br>(−1.020) | −.0002<br>(.062) |
| Republican challenger expenditures | −.103***<br>(−3.742) | −.169***<br>(−4.464) |
| Pardon disapproval/finances worse | .243**<br>(1.968) | .583**<br>(3.329) |
| Incumbent democrat | .449***<br>(3.831) | 1.026***<br>(5.935) |
| Constant | 1.7603***<br>(8.927) | 1.105***<br>(4.602) |
| $\hat{R}^2$ | .21 | .54 |
| Percent predicted correctly | | |
| Probit | 81.1 | 81.1 |
| Null | – | 56.1 |
| Spearman's rho | .42 | – |
| $N$ | 467 | 467 |

Entries are unstandardized probit coefficients; $z$-scores in parentheses.

$^{*}p < .10$.
$^{**}p < .05$.
$^{***}p < .01$.

tween these groups. Traditional voting research has stressed continuity, primarily through the prism of party identification. We would expect, however, that switchers would be far less influenced by the simple force of party—and thus also more likely to be affected by short-term factors such as Watergate and economic grievances.

## Results of the Analysis

The estimated coefficients in the probit analyses are presented for all voters in Table 2, measured as both simple vote choice and as a ranked indicator, with the categories in the latter being 1 for new voters who voted Republican or for 1972 nonvoters or Democratic voters who voted Republican in 1974, 2 for consistent Republican voters, 3 for consistent Democratic voters, and 4 for new voters and for previous nonvoters or Republican voters in 1972 who voted Democratic in 1974. The results for switchers and standpatters are presented in Table 3. Despite some evidence that the four-category variable for all voters is not strictly ordinal,[9] the pattern of significance levels for the dependent variables in Table 2 is quite similar. Because the variables have different scales, however, the probit coefficients cannot be compared. What we find is that both sets of equations are heavily driven by party identification.

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[9] Because we were not sure that the four-category vote change variable for all voters was strictly ordinal, we also ran a logit model (which does not assume ordinality) for this variable. Logit, however, is more difficult to interpret because it yields $(k-1)$ sets of coefficients where $k$ is the number of categories in the dependent variable. Hence, instead of eight probit coefficients, we have 24 logit estimates. Briefly, the logit results suggested that only party identification and spending by GOP challengers were significant for all groups. Most errors in prediction occurred among switchers, not surprisingly. Bot the logit model yielded a much higher rank order correlation (.7) than the probit analysis did, which suggests that the variable is not strictly ordinal. However, the similarity in results for the two constructs for all voters in Table 2 suggests that comparisons can be made, if with some caution.

Contrary to what we expected, there are no significant effects found for party identification change, change in trust, or change in the Nixon feeling thermometer.

The Jacobson and Kernell thesis argues that campaign expenditures, particularly by challengers, would play a major role in vote choice. Republican challenger expenditures were strongly related to both variables, as was incumbent Democrat. The more money Republican challengers spent, the more votes they received. Thus, whereas Democratic incumbents had a larger advantage over their Republican challengers in 1974 than in other recent years (cf. Table 1), what money GOP upstarts did have apparently was put to good use.

However, this result runs directly counter to the Jacobson-Kernell thesis. *If anyone should have been more advantaged in 1974, it should have been Democratic challengers.* Yet spending by Democratic challengers did not yield significant coefficients for any of our four dependent variables, either in combination with expenditures by Republican candidates or when estimated by itself. Another variable designed to test the Jacobson-Kernell thesis measures the change from 1972 to 1974 of Democratic to all expenditures for contested races in the sample. The logic of this formulation is straightforward. The variable measures the relative improvement in Democratic candidates' financial resources from one election to the next. Thus, linking it to vote change provides a measure of the elasticity of campaign expenditures by Democrats. The surge in Democratic coffers in 1974, as indicated by the Jacobson-Kernell data, should have a pay-off among voters. The variable did yield significant coefficients when considered alone, but not when expenditures of Republican challengers were also entered. When each was entered separately, the GOP challenger expenditure coefficient had consistently higher *z* scores.

The variable for Watergate and inflation does yield significant coefficients for both equations for all voters in the panel. In contrast to previous researchers, then, we have found that Watergate and the economy affected vote choice in 1974, whether we consider the simple congressional vote (as has traditionally been done) or a more complex vote change variable. Overall, the equation

**Table 3. Probit Analyses for 1974 Voters on Congressional Elections: Standpatters and Switchers**

| Predictor | Switchers | Standpatters |
|---|---|---|
| Party ID | −.278**<br>(−2.178) | −1.212***<br>(−10.956) |
| Party ID change | −.186*<br>(−1.389) | −.167<br>(1.326) |
| Change in trust | .165**<br>(2.116) | −.027<br>(−.399) |
| Change in Nixon feeling thermometer | −.004<br>(−.985) | .003<br>(.817) |
| Republican challenger expenditures | −.173***<br>(−3.478) | −.131**<br>(−1.833) |
| Pardon disapproval/finances worse | .532**<br>(1.835) | .623***<br>(2.495) |
| Incumbent Democrat | .657***<br>(2.357) | 1.314***<br>(5.176) |
| Constant | .177<br>(.447) | 1.677***<br>(5.073) |
| $\hat{R}^2$ | .25 | .70 |
| Percent predicted correctly | | |
| Probit | 69.3 | 87.8 |
| Null | 57.9 | 55.4 |
| *N* | 140 | 327 |

Entries are unstandardized probit coefficients; *z*-scores in parentheses.

$^{*}p < .10$.
$^{**}p < .05$.
$^{***}p < .01$.

for the 1974 vote performs much better than that for the four-category vote change variable. The latter equation has an $\hat{R}^2$ of only .21 with a rank order correlation between actual and predicted votes of .42. For the simple congressional vote, the $\hat{R}^2$ is .54; 81.1% of the cases are predicted correctly, compared to a benchmark of 56.1% for a null model based upon assuming all voters would cast Democratic ballots. The estimated coefficient of determination is only .07 lower than Fiorina's (1981, p. 165) for the same dependent variable with a similar number of cases (454), but which uses 17 predictors instead of our seven. Indeed, our percent predicted correctly is even marginally higher than Fiorina's (80.6).

The greatest vote surge for the Democrats for both conceptualizations of all voters, other than party identification, came not for Democratic challengers but for Democratic incumbents. This result seems both partly reasonable and partly anomalous. On the one hand, it is very plausible that Democratic incumbents did do better in 1974 than in 1972. On the other hand, the failure to find corresponding gains for Democratic challengers (but cf. Fiorina, 1981, p. 162) leads us to inquire precisely where the votes that gave the Democrats 49 additional seats came from. There are three possibilities. First, we could find very different effects for switchers and standpatters. Second, it might be argued that the combined effects of Watergate and inflation, together with change in party identification as a mediating force, produced all the change needed to account for the behavior of switchers to Democratic challengers. Third, there is the possibility that these results tell us something about the nature of the sample. We shall investigate these possibilities after considering the results for switchers and standpatters.

The equations comparing switchers and standpatters in Table 3 suggest that there are some different patterns for these voters. The coefficients for longer-term party-related variables are much stronger, as might be expected, for standpatters. Party identification overwhelms all other predictors for standpatters, based upon the *z*-scores for the coefficients. It remains important for switchers, but far less so. This is not surprising, because standpatters voted the same way in 1972 and 1974 and presumably have more durable long-term party ties. Similarly, incumbent Democrat has stronger *z*-scores for standpatters than for switchers. On the other hand, change in party identification has the wrong sign for standpatters, but is significant for switchers at the .10 level. Republican challenger expenditures is another short-term party effect which is stronger for switchers, although it is significant for standpatters as well.

The pattern for Watergate-related variables from the probit equations is mixed. Initially, it appears that the pardon-disapproval and finances-worse construct has a stronger effect for standpatters. It is not surprising to find that traditional Democrats who stayed with their party in 1972 and 1974 would be affected by such concerns, but we would expect switchers to be motivated at least as much by these short-term issues which were so central to the public debate in 1974. When we examine the probabilities of voting Democratic in 1974, we shall find more support for this thesis. There are no significant coefficients for changes in the Nixon feeling thermometer. However, for switchers in particular there is a moderate correlation between this variable and the pardon-disapproval and finances-worse one; a coefficient significant at .10 is found when the latter is dropped in the equation for switchers. Finally, an insignificant negative coefficient is found for change in trust for standpatters, following the results for all voters. For switchers, a significant positive coefficient is obtained. A positive coefficient, however, should not be all that surprising, because trends in trust follow short-term political situations (Miller, 1974). Positive evaluations of the government ("the system worked") might well have emerged from the long trauma of Watergate. It seems eminently reasonable that voters would reward the Democrats, particularly those running for the institution that had forced Nixon from office, for their renewed faith in the system. Those voters who tossed out GOP incumbents for supporting Nixon may have very well thought that their Representatives were far more antisystem than the electorate (cf. Wright, 1977). Overall, the results for standpatters are stronger than for switchers. The respective $\hat{R}^2$ values are .70 and .25, with 87.8% and 69.3% of the cases predicted correctly. The difference in explanatory power of the equations is accounted for by the differential impact of party identification.[10]

## All Voters, Switchers, and Standpatters

Because the probit coefficients have no ready interpretation, we need another way of comparing the results obtained for all voters, switchers, and standpatters. We begin with the fact that the predicted probit scores for each case are *z*-scores which can be converted into probabilities. For each voter, we obtain probabilities at the two extreme situations and these probabilities are ag-

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[10] Unfortunately, the variables that allow us to assess dimensions of incumbency were not included in the National Election Studies until 1978.

Table 4. Probabilities for Predictors for the 1974 Congressional Vote for Democratic Candidates[a]

| Predictor | All Voters | Switchers | Standpatters |
|---|---|---|---|
| Party ID | .809/.265/ .541 | .666/.471/ .195 | .868/.191/ .677 |
| Party ID change | .525/.586/-.061 | .754/.450/ .304 | .496/.609/-.140 |
| Change in trust | .637/.497/ .140 | .718/.386/ .332 | .506/.552/-.046 |
| Change in Nixon feeling thermometer | .567/.564/ .003 | .667/.457/ .210 | .515/.600/-.085 |
| Pardon disapproval/finances worse | .598/.446/ .152 | .622/.437/ .185 | .581/.464/ .117 |
| Incumbent Democrat | .693/.430/ .263 | .676/.461/ .215 | .683/.467/ .216 |
| Republican challenger expenditures | .604/.064/ .540 | .671/.029/ .642 | .570/.198/ .372 |
| Republican challenger expenditures—adjusted | .604/.257/ .347 | .671/.204/ .467 | .570/.410/ .160 |
| Republican challenger expenditures—doubly adjusted/incumbent Democrat controlled | .727/.365/ .362 | .755/.275/ .480 | .696/.536/ .160 |
| Four Watergate variables | .686/.470/ .216 | .972/.205/ .767 | .498/.581/-.083 |

[a] First entries reflect signs of probit coefficients for switchers and reflect predicted greater likelihoods of voting Democratic in 1974; the third entries are changes in probabilities and are underlined.

gregated and averaged across all voters in the (sub-) sample(s), as described in note 6 above. The results are displayed in Table 4. We use the signs of the coefficients for switchers to determine the direction of the impact of the probabilities. The entries in each column in the table are: 1) the average probability, given the values for each case on uncontrolled variables, that the voter would cast a Democratic ballot when the predictor is posited to be favorable to a Democratic vote; 2) a similar measure for the situation when the predictor is posited to be favorable to a Republican vote; and 3) the difference between 1) and 2), which is the marginal effect of the variable(s) in question on 1974 voting behavior.

The critical differences are found between switchers and standpatters. For the latter group, party identification clearly overwhelms all of the other predictors. Controlling for all of the other variables, a Democratic standpatter has a .868 probability of voting for that party in 1974, whereas a GOP standpatter has only a .191 probability of casting a Democratic ballot, for a difference in probabilities of .677. In contrast, a Republican switcher has a .471 probability of voting Democratic and the difference in probabilities, or the marginal impact of party identification on switchers, is only .195, less than a third of that found for standpatters. The variable incumbent Democrat has similar effects for the two subsamples, however. What is most fascinating is the pattern of the four short-term Watergate-related variables: change in party identification, change in trust, change in the Nixon feeling thermometer, and the combined pardon-disapproval and finances-worse construct. For switchers, the marginal impact of the first two variables is over 30%. A switcher who moved from strongly Republican to strongly Democratic will thus be 30.4% more likely to vote Democratic in 1974 than one who moved the other way. But a standpatter whose party identification shifted strongly toward the Democrats would actually be 14% more likely to vote Republican than a standpatter whose allegiance shifted toward the GOP! The change in probability for the trust variable also moves in a contrary direction to that for standpatters, although the value is quite small (−.046).

Change in the Nixon feeling thermometer yields a marginal impact of over 20% for switchers, compared to a small negative change for standpatters. The pardon-disapproval and finances-worse variable does, as expected, yield positive impacts for both switchers and standpatters. Although the .185 values for changes in probabilities is the smallest impact observed for any of the seven predictors for switchers, it is still almost 7% greater than the change for standpatters. When the four short-term Watergate variables are considered together, their joint marginal impact on standpatters is barely negative (−.083), but strongly positive (.767) for switchers. A switcher with pro-Democratic values on all four variables will have .972 probability of casting a Democratic ballot in 1974, compared to a .204 probability for a switcher with pro-Republican values on all four variables—controlling for the effects of party identification and spending by Republican challengers. What is important here is not the magnitude of the effects for switchers, but rather the great difference in results for the two samples. The estimated joint impacts are "too high" for switchers because there are very few voters in the sample who have either uniformly pro-

Democratic or pro-Republican values on all variables: the highest zero-order correlation across any of the samples between any pair of Watergate variables is $-.22$ for the relationship between change in the Nixon feeling thermometer and the pardon and finances-worse variable for switchers. The results for standpatters must be tempered slightly because it simply goes against all logic to maintain that Democrats who were incensed at Nixon in 1974 took out their rage by being slightly more likely to vote for Republican candidates for Congress. Note first that the "reverse" Watergate effects for standpatters are small anyway and that they are certainly overwhelmed by an even deeper-seated dislike for Nixon encapsulated in the power of one's party identification.

Even as the many other factors of the Watergate year worked against Republicans, those minority party challengers who were well funded scored quite well among both standpatters and particularly switchers. Money may well have been the difference between simply losing, as did almost all Republican challengers, and humiliation. Standpatters in districts with well-funded challengers were 37.2% more likely to vote Republican than they were if the challengers spent nothing, whereas the difference was 64.2% for switchers. But the construction of this variable can readily lead to erroneous conclusions. First, there was one district in the sample in which a Republican challenger spent $155,580; both a switcher and a standpatter resided in that constituency. But the next highest spending total was $85,050 for switchers and $68,040 for standpatters. The highest value, then, was clearly an outlier, and to simulate an outcome based upon every GOP challenger spending that amount of money is little more than an exercise of wishful thinking for the Republican congressional campaign committee in 1974. When we adjust the figures at the upper extremes, more reasonable results are found: .467 for switchers, still the largest marginal impact for this subsample, and .160 for standpatters.

However, there is an additional problem with this simulation. The lower bound of the variable is zero, which occurs whenever the race is not between a Republican challenger and a Democratic incumbent. Thus, to get a more accurate measure of the impact of Republican challenger expenditures, we must treat each race as if it involved such a contest. To do so, we first set the variable incumbent Democrat equal to one for all cases. Then, we must derive an alternative lower bound for expenditures. The data indicate that $2000 is such a floor, because more than just a handful of candidates were bedeviled with this low level of money in 1974. Doubly adjusting the bounds and controlling for incumbent Democrat only serves to show how robust the results are. The differences in probabilities barely change at all. However, the probabilities obtained at each bound do show a greater propensity for Democratic voting than the earlier constructs for this variable.

The results for all voters, which would be most directly comparable to previous studies, are once again generally a mixture of the findings for the two subsamples, but mostly closer to the standpatters. The two variables with the greatest marginal impact are party identification and Republican challenger expenditures. Close behind is incumbent Democrat. Nevertheless, our specification does yield noticeable effects for Watergate and the economy: the pardon-disapproval and finances-worse variable and change in trust each increase the probability that a voter will cast a Democratic ballot by approximately 15%. The joint impact of the four Watergate variables is a change in probability of .216. Only change in the Nixon feeling thermometer and change in party identification yield no measurable pro-Democratic impact.

The failure to find Watergate effects in previous studies can be traced largely but not exclusively to the failure to analyze switchers and standpatters separately. There are such effects for all voters, however, and this suggests that the question cannot be reduced entirely to differences in the subsamples. Specification is also a key issue, as are methods of analysis. The separate analyses for switchers and standpatters do not establish simply that there are Watergate effects, but rather that for some voters these effects may be much longer than for others—and, indeed, that for these variables the results for all voters may be driven by a relatively small subsample of voters.

## The Nature of the Sample

Most of our results make sense in light of the conventional wisdom about the 1974 elections. However, in battles which seemed to highlight the success rate of Democratic *challengers,* we find that the variable "incumbent Democrat" was quite significant and had a considerable marginal impact as well. Did we miss something important about the 1974 elections, or is there another explanation for these findings? Earlier, we offered three possible explanations for how this variable might have considerable effects on voting behavior. First, there is the possibility that the voting behavior of standpatters and switchers was asymmetric with respect to incumbents and challengers. The probit equations suggested that this might indeed be the case, because standpatters

**Table 5. District Outcomes and Panel Voters for Races Involving Incumbents with Major Party Challengers in 1974: Democrats**

| | Percent of Contested Races with Democratic Incumbent | | | Percent of All Contested Races | | |
|---|---|---|---|---|---|---|
| Incumbent's Percentage of Two-Party Vote | All Districts | Panel Voters | Switchers | All Districts | Panel Voters | Switchers |
| 60+ | 85.9 | 66.5 | 56.3 | 33.8 | 33.5 | 30.2 |
| 55-59.9 | 7.0 | 11.9 | 16.5 | 2.8 | 6.0 | 8.9 |
| 50-54.9 | 5.3 | 7.3 | 12.6 | 2.1 | 3.7 | 6.8 |
| 45-49.5 | 1.2 | 7.9 | 6.8 | 0.5 | 4.0 | 3.6 |
| Below 45 | 0.6 | 6.3 | 7.8 | 0.2 | 3.2 | 4.2 |
| Total (%) | 100.0 | 99.9 | 100.0 | 39.4 | 50.4 | 53.7 |
| *N* | 171 | 302 | 103 | 171 | 600 | 192 |

Sources: Center for Political Studies, National Election Studies 1972-1974 Panel; *Congressional Quarterly Almanac 1974,* pp. 857-864.

had a much higher *z*-score for this variable than did switchers. However, our analysis of the marginal impact of "incumbent Democrat" yielded virtually identical changes in probabilities for the two subsamples. Our second explanation was that the equation for switchers might fully account for the surge for challengers. The $\hat{R}^2$ value for the switchers' equation certainly seems capable of improvement. Thus, we must consider the third explanation: The nature of the sample led to the strong coefficients for incumbent Democrat.

Tables 5 and 6 report breakdowns of the distribution of the sample and the corresponding population (in terms of congressional districts) by incumbents' percentages of the two-party vote in 1974. Again, uncontested districts are eliminated; all 56 had Democratic incumbents. The remaining districts left as sampling possibilities numbered 171 with Democratic incumbents, 166 with Republican incumbents, and 42 open seats. However, the sample greatly overrepresented constituencies with Democratic incumbents: there were 302 voters residing in districts with Democratic incumbents compared to 191 represented by GOP incumbents and 107 in open-seat districts. The ratio of respondents residing in contested districts with Democratic incumbents to those with GOP members was 1.32 in the sample compared to 1.03 in the population.[11]

**Table 6. District Outcomes and Panel Voters for Racces Involving Incumbents with Major Party Challengers in 1974: Republicans**

| | Percent of Contested Races with Democratic Incumbent | | | Percent of All Contested Races | | |
|---|---|---|---|---|---|---|
| Incumbent's Percentage of Two-Party Vote | All Districts | Panel Voters | Switchers | All Districts | Panel Voters | Switchers |
| 60+ | 33.7 | 38.2 | 43.9 | 12.9 | 12.2 | 13.0 |
| 55-59.9 | 20.5 | 28.3 | 33.3 | 7.8 | 9.0 | 9.9 |
| 50-54.9 | 23.5 | 18.3 | 15.8 | 9.0 | 5.8 | 4.7 |
| 45-49.5 | 16.3 | 13.6 | 7.0 | 6.2 | 4.3 | 2.1 |
| Below 45 | 6.0 | 1.6 | 0.0 | 2.3 | 0.5 | 0.0 |
| Total (%) | 100.0 | 100.0 | 100.0 | 38.2 | 31.8 | 29.7 |
| *N* | 166 | 191 | 57 | 166 | 600 | 192 |

Sources: See Table 5.

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[11] These respondents (in Tables 5 and 6) include only voters. The *N*s vary from those in the equations estimated in Table 2 because we employed listwise deletion of missing data. It should also be noted that the congressional district was *not* the primary sampling unit in 1974. However, that does not obviate our claims. For

Not only did the sample include proportionately too many voters from districts with Democratic incumbents, but it also overrepresented districts with marginal Democratic incumbents and of the three majority party members who lost. The relevant data are in Table 5. Only 1.8% of Democratic incumbents lost in 1974, but 14.2% of the panel voters and 14.6% of the switchers were from such districts. When we shift our focus to *all* districts, Democratic incumbents who were defeated constitute a miniscule 0.7% of the population, but 7.2% of all panel voters and 7.8% of all switchers reside in these constituencies. In contrast, voters in Republican districts with incumbents who scraped by with less than 55% of the vote or who were defeated are underrepresented by substantial margins. The data are presented in Table 6. Although 22.3% of districts with GOP incumbents fell to the Democrats in 1974, voters in those constituencies constituted only 15.2% of the panel and just 7% of the switchers. Republican members who lost were four times as numerous across all districts as the percentage of switchers in those constituencies. There were, then, just four people interviewed who switched votes from 1972 to 1974 and resided in districts that tossed out Republican incumbents!

Given these sample biases, it is straightforward to understand why "incumbent Democrat" has significant coefficients in all equations estimated in Tables 2 and 3. There may be some attraction to bring even more votes to already successful Democrats, but the strength of the coefficients is surely overestimated. If this is the case, then it is likely that spending by Republican challengers, which would limit the vote-gathering possibilities by incumbent Democrats, is also estimated to be stronger than it should be.

The bias in the sample undoubtedly attenuates the coefficients for Watergate and economic grievances. Almost twice as many of the switchers (103 to 57) lived in districts with Democratic incumbents rather than Republican members. Thus, the sample underestimates the impact of throwing the rascals out. Someone who voted for a Republican challenger to a Democratic incumbent in 1972 and then cast a ballot for the incumbent two years later contributed less to the shock value of the 1974 election than did another voter who helped bring down a Republican incumbent he or she supported just one election ago. Because the punishment of Republican incumbents tended to be somewhat selective—toward the more pro-Nixon, conservative members (cf. Wright, 1977), one can make a good case that the sample bias will seriously underestimate the effects of Watergate and economic complaints for switchers.

## Conclusion

The politicians and the pundits did not miss the message of the 1974 congressional elections, as well as those for lesser offices. But it would be a mistake to argue that those contests were nothing more than a referendum on Watergate and the economy. For most voters, those who traditionally rely upon party identification and who cast ballots for the same party or candidate election after election, voting is a ritual. That ritual may very well have policy content and is likely to have some retrospective component as well. To paraphrase Key, standpatters are not fools either. But their voting behavior is driven by loyalties to party and candidate which stem from either deep-seated policy agreement or a more general affect that transcends most national political trends. The importance of such loyalties was never more apparent than in 1972, when Nixon scored a massive landslide but the Democrats retained control of the Congress.

Periodically, however, there are vote swings of such a magnitude that they change the balance of power in the halls of Congress. These are often, although not exclusively, mid-term contests (1946, 1958, 1966, 1974). The ripple effects of the 1974 election, like all landslides, occurred because of changes at the margin by voters (cf. Fiorina, 1977). The Democratic share of the two-party congressional vote rose just under 6% from 1972 to 1974. It is this change that commands our attention and that politicians and pundits follow. As they know, retrospective voting didn't start with Watergate. But neither did it terminate with the escalation of the Vietnam War in 1966.

The "uncovering" of Watergate and economy effects does not suffice to undermine the Jacobson-Kernell thesis about strategic politicians. If the coefficients on "incumbent Democrat" and expenditures by Republican challengers were more robust, they would effectively put that formulation to rest. At the least, one must now argue that there was an identifiable Watergate-economy effect in 1974 that, if anything, was underestimated. Given the reinterpretation of the Jacobson-Kernell expenditure data in Table 1 above and the identification of such effects for switchers, there is probably no need to reject the conventional wisdom about what the 1974 elections settled. The scope of the

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an argument about the 1978 sample, the first to use the congressional district as the primary sampling unit, and its biases for analyzing Senate races, see Mann and Wolfinger (1980). The pro-incumbent bias on CPS samples is discussed by Eubank and Gow (1983) and Gow and Eubank (1984).

landslide was too broad and deep to require that strategic politicians be available everywhere to exploit Democratic victory hopes. However, it does seem very plausible to argue that such enhanced prospects did bring out the Democrats' best and brightest; we cannot divorce the argument about strategic politicians from their political environment. Just as Mayor Fiorello LaGuardia of New York claimed that he could win reelection "on a laundry ticket," many far lesser candidates even in 1974 benefited from their simple identification with the Democratic Party. Much of the evidence is anecdotal, but the "Watergate babies" who entered the Congress in 1975 included many amateurs (Granat, 1984).

Altogether, our results indicate that the Democratic surge in 1974 can be explained by a framework that recognizes vote switchers, contributors, and candidates as making strategic investment decisions. Switchers whose trust in government increased as a result of the Democrats forcing Nixon from office but who had either Watergate or economic grievances against the Republicans moved toward the Democrats in 1974. To the extent that the results for the variable "incumbent Democrat" are robust (cf. Fiorina, 1981, pp. 164-167), such voting behavior primarily rewarded sitting members of the majority party in Congress for opposition to administration policies. Because there were substantially more Democratic than Republican incumbents, this voting pattern merely reflects the opportunity structure available to voters. Democratic candidates across the board fared better relative to their Republican opponents in terms of campaign expenditures in 1974 compared to 1972. But this does not imply that contributors simply poured money into the campaign coffers of Democratic incumbents in 1974. Indeed, the average dollar amount available to those incumbents, not even adjusted for inflation, declined from 1972 to 1974 (Jacobson & Kernell, 1981, p. 41)! However, the decline in contributions to Republican challengers was much more precipitous, so that Democratic incumbents scored the largest percentage gain in funds expended from 1972 to 1974. Simply put, contributors realized that most Democratic incumbents did not need substantial sums to win, even to win big, in 1974, and that money given to their GOP challengers might increase Republican vote totals but would not save such challengers from defeat.

On the other hand, Republican incumbents averaged almost $30,000 more in expenditures in 1974 compared to 1972 and still far outdistanced their Democratic challengers in terms of funds available (Jacobson & Kernell, 1981, p. 41). But, relatively, Democratic challengers had far more funds in 1974 than two years earlier. Yet the failure of Democratic challenger expenditures to reach significance in our equations suggests that there were larger macropolitical factors at work in accounting for vote change in 1974; if anything, the investment decisions of contributors reflected the perception that money would not suffice to counter national trends. Being a strategic politician in 1974 meant being a Democrat; a Democrat with widespread name recognition, either as an incumbent or a challenger with prior elective office, only compounded the difficulties faced by Republican opponents.

The task ahead is to determine whether party identification change can be modelled as an instrumental variable in the same way that Fiorina (1981) has conceptualized lagged identification in his formulation of retrospective voting. Brody (1977) has suggested that this ought to be the case, but the correlations we have uncovered so far between party identification change and other variables are all very modest. We shall pursue this issue in other articles.

There are larger theoretical issues raised by our analysis, ones that are not confined to the interesting but limited question of what drove voters in 1974. Are standpatters and switchers motivated by different factors in other elections as well? If so, which seems likely, what does this imply for the much-heralded decline in the effect of party identification on the vote? Might party identification be alive and well for standpatters (habitual partisans) but weakening for switchers? Might the weakening of party identification be tied to a larger number of switchers in recent years than in the past? The impact of change in party identification upon 1974 vote choice for switchers was quite strong and suggestive of how simple party identification might, for some segments of the electorate, be a reflection of short-term rather than long-term attachments. It might not be surprising to find that switchers behave differently from standpatters, but the magnitudes of the differences we have found suggest that it is inappropriate to use the same models to account for the voting behavior of these two very different groups.

## Appendix

*Change in trust.* Ranges from −5 to +5. It is the difference between two summed indices based on V570 to V574 (1972) and V2229 to V2233 (1974).

*Democratic incumbent in 1974.* Incumbent Democratic representative in the district = 1; otherwise = 0. (Coded from Gary Jacobson's data.)

*Change in Nixon evaluation.* Difference between thermometer ratings in 1974 and 1972. Ranges from −98 to +60. Computed from V2354 (1974) to V702 (1972).

*Change in party identification.* Change in the strength of party identification between 1972 and 1974. (Computed from V140, V2204. Ranges from +3 to −3.)

*Party identification in 1974.* 1 = Democrat; 2 = Independent, 3 = Republican (V2204).

*Evaluation of Nixon's pardon.* 1 = disapproval, 0 = approval (V2166).

*Joint evaluation of personal financial condition and Nixon pardon.* 0 = approve of pardon and personal financial situation same or better; otherwise = 1. (Computed from V2166, V2313).

*Republican challenger expenditures in 1974.* Reported campaign expenditures by Republican challengers to Democratic incumbents in 1974. 0 if Republican incumbent or open seat. (Jacobson data).

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# Expectations and Preferences in Presidential Nominating Campaigns

LARRY M. BARTELS
*University of Rochester*

*Survey data from the preconvention waves of the 1980 National Election Study are used to estimate the effect of expectations about who will be nominated on respondents' own preferences. The results confirm the conventional belief that bandwagons play an important role in nominating campaigns; at the same time, they suggest that the dynamics of the nominating process may be more subtle than simple bandwagon models would indicate. First, preferences are strongly and consistently projected onto expectations, making the relationship of central interest a reciprocal one. Second, the bases of candidate choice appear to change systematically with political circumstances. In close, volatile campaigns, support for bandwagon candidates (like George Bush in early 1980) is based largely on favorable expectations and on relatively general, diffuse political evaluations (e.g., "leadership"). By comparison, when expectations about the nomination are very one-sided, their impact on preferences approaches zero, and more specific, substantive political evaluations become increasingly important.*

The meteoric rise to prominence of Gary Hart is the latest in a series of object lessons concerning the importance of momentum in the contemporary presidential nominating process. Like George McGovern, Jimmy Carter, George Bush, and John Anderson before him, Hart capitalized on better-than-expected performances early in the caucus and primary season to break out of the pack, attracting remarkably broad support from an electorate that had barely recognized his name a few months earlier.

A variety of journalistic and academic observers have documented the apparent impact of momentum in recent nominating campaigns.[1] However, most of these accounts have focused solely on the broad dynamics of the phenomenon; few provide any evidence, or even speculation, about the political character of momentum or about the nature of the individual-level behavioral mechanisms underlying the aggregate-level phenomenon.[2] Thus, in the wake of Hart's remarkable success, pundits and analysts found themselves groping for answers to the same kinds of questions they previously asked about Carter, Bush, and others. How did he take off? Was his momentum based mostly on support for his new ideas, on strategic calculation, or on the pure excitement of his rush to the forefront of the race? What are the implications of his success for our understanding and evaluation of the institutions we use to select our presidential nominees?

As a first step toward answering these and similar questions, I propose and test a microlevel model in which the preferences of potential voters are determined in part by their expectations about who will be nominated. A "bandwagon" effect of this sort—however rational or visceral—would account for the main features of the aggregate-level phenomenon of momentum by causing a candidate whose chances of being nominated are improving to attract increasing support, thus improving his chances still further, attracting even more support, and so on over time.[3] In this sense, my model provides a useful individual-level test of the importance of momentum in nominating campaigns. At the same time, my focus on the individual political behavior underlying the phenomenon of momentum makes it possible to embed the impact of expectations on preferences within a more general model of political perceptions and preferences in the preconvention period, and thus to specify more precisely than has

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[1] For relevant analyses of recent campaigns, see Schram (1977), Witcover (1977), Matthews (1978), Aldrich (1980), Marshall (1981), and Bartels (1983).

[2] A notable partial exception was provided by Shanks and Palmquist (1981).

[3] For a mathematical analysis of the dynamics of this process, see Aldrich (1980b).

heretofore been possible the nature and role of "bandwagons" in the nominating process.

## Data and Models

In 1980, for the first time, a CPS National Election Study provided extensive information about public preferences and expectations during a presidential nominating campaign. The first part of this multiwave survey consisted of interviews with more than 1000 respondents in late January and February 1980, after the Iowa caucuses but before the New Hampshire primary. It thus provides a useful source of information about individual attitudes and expectations during the crucial early phase of the nominating campaign. A second cross-section of similar scope conducted in April makes it possible to compare early attitudes and preferences with those prevailing well into the sequence of primaries and caucuses.

In both of these cross-sections, and for both parties, the joint distribution of expectations and preferences is strikingly consistent with the bandwagon hypothesis. Among Democrats and Independents, those who expected Carter to win the Democratic nomination were much more likely than other respondents to prefer Carter as the Democratic nominee.[4] In the January-February survey, 63% of those who *expected* Carter to win also *wanted* him to win, whereas only 7% of those who did not expect him to win preferred him themselves. Similarly, in the April survey, Carter was the first choice of 52% of the respondents who expected him to be nominated, but of only 9% of those who did not expect him to be nominated. Among Republicans and Independents, those who expected Reagan to win were also substantially more likely to prefer him themselves; the proportions were 42% versus 9% in January-February and 45% versus 1% in April.

These congruences between expectations and preferences are certainly consistent with the hypothesis that expectations about who will win the nomination influence the preferences of potential voters during the primary season. Indeed, these bivariate relationships between preferences and expectations are as strong or stronger than the relationships between preferences and any of the usual range of variables used to explain preferences, such as ideology, job ratings, issue positions, or personality evaluations. However, in estimating the causal impact of expectations on preferences, it is necessary to take account of an obvious alternative explanation for the relationship between the two variables: potential voters may first decide which candidate they prefer, and then project these preferences onto their expectations about the outcome of the nominating contest, using selective perception or wishful thinking to convince themselves that their favorite is doing well regardless of the real political situation. Such projection effects have previously been documented, not only for expectations about election outcomes (Berelson, Lazarsfeld, & McPhee, 1954, p. 289), but also for perceived issue positions of candidates (Berelson, Lazarsfeld, & McPhee, 1954, chap. 10; Markus & Converse, 1979; Page & Jones, 1979) and groups (Brady & Sniderman, 1983). These results are substantively important for the revisions they suggest in our inherited theories of political behavior. But they are also methodologically important: even if the phenomenon of projection was of no interest in its own right, we would still have to take it into account in order to estimate accurately other political relationships of interest.

In the present case, if we ignore the fact that primary season expectations are, in part, projections of the potential voters' own preferences, we are bound to overestimate the true strength of the reciprocal bandwagon effect of expectations on preferences. The only way to avoid this bias is to construct a model that explicitly incorporates the simultaneous reciprocal impacts of the bandwagon and projection effects. In the remainder of this article I develop just such a model, based on the assumptions that preferences are influenced by expectations (the bandwagon effect) and by comparisons of the competing candidates on the issues (broadly defined), and that expectations are influenced by preferences (the projection effect) and by social characteristics measuring political awareness. The causal relationship between expectations and preferences is thus allowed to work in both directions, with each of these central variables also determined in part by other variables that can be treated for present purposes as exogenous.

The specific variables included in my analysis of the 1980 data are shown in Figure 1 for Democrats and in Figure 2 for Republicans. The variables on the left side of each figure are those assumed to affect preferences, but not to affect expectations (except indirectly through preferences). The variables on the right side of each figure are those assumed to affect expectations, but not to affect preferences (except indirectly through expectations).

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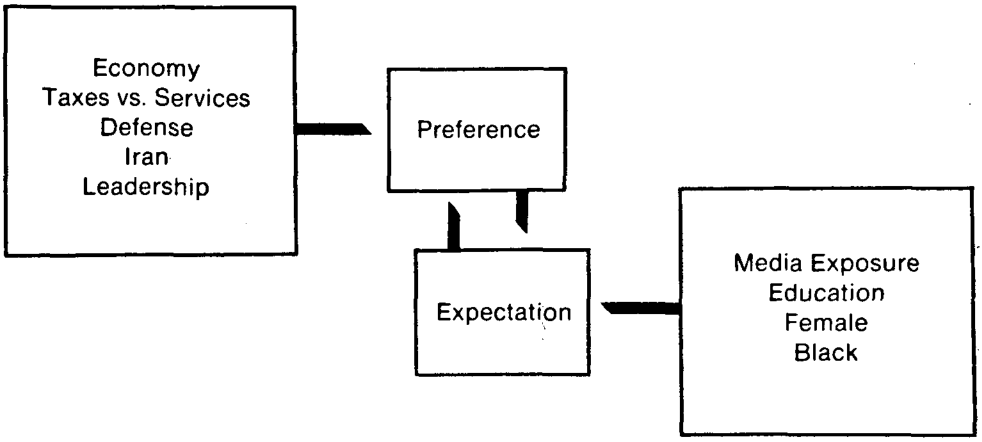
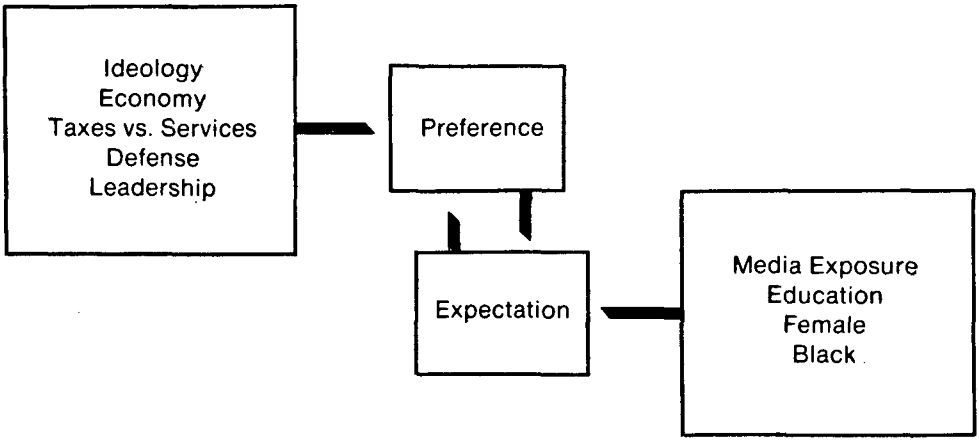
[4] Throughout this article, analyses of the Democratic campaign are based only on responses from Democratic identifiers, Independent Democrats, and "Pure" Independents. Analyses of the Republican campaign are based only on responses from Republican identifiers, Independent Republicans, and "Pure" Independents.

Figure 1. Causal Model for Expectations and Preferences: Democrats


Economy
Taxes vs. Services
Defense
Iran
Leadership
Preference
Expectation
Media Exposure
Education
Female
Black

Given the relative paucity of previous theoretical or empirical analyses of preconvention preferences, the exact specification of these models must unavoidably be to some extent a matter of guesswork. Additional exogenous variables could have been included (especially on the preference side of the models); where they would not have altered the results and interpretations presented here, I have excluded them.[5] This parsimonious approach seems appropriate because my goal is to investigate a specific causal relation-

Figure 2. Causal Model for Expectations and Preferences: Republicans


Ideology
Economy
Taxes vs. Services
Defense
Leadership
Preference
Expectation
Media Exposure
Education
Female
Black

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[5] Many of the exclusions in the models estimated here are based on preliminary empirical experimentation. For example, education and race were dropped from the preference side of the model because their effects were negligible after controlling for issues, leadership, and expectations. Several other variables (e.g., age, frequency of church attendance, attitudes about energy and the environment) were dropped from the analysis completely, because they appeared not to have distinct effects of sufficient strength to be worth retaining. On the other hand, the major argument against including a variety of the available "personality" evaluations is a theoretical one: these evaluations—when they exist at all—are unlikely to be exogenous, so that adding them to the models would complicate the analysis greatly without providing much promise of additional substantive insight.

ship, rather than to construct a general explanation of political attitudes in the preconvention period. In any event, the specifications in Figures 1 and 2 do tap the dimensions of political perception and evaluation most commonly stressed in accounts of the 1980 nominating campaigns (e.g., Harwood, 1980).

The usual method for estimating parameters in models like the ones proposed here is two-stage least squares. In this case, however, the situation is complicated by the fact that expectations and preferences are both measured as dichotomies; for example, Carter is named as the most likely nominee or not, and Bush is a respondent's first choice or not.[6] These dichotomous responses presumably reflect underlying continua of expectations and preferences which, though unobserved, are the real objects of analysis. In order to estimate causal relationships involving these underlying continua, I have modified the usual two-stage least squares procedure by employing nonlinear logit models in both the first and second stages of the analysis. The first stage uses exogenous variables to construct purged estimates of each respondent's position on the underlying preference and expectations continua, and the second stage estimates the parameters of the structural model relating the underlying continua to each other and to the exogenous variables.[7]

### Iowa to New Hampshire: Democrats

Having outlined a model of preferences and perceptions in nominating campaigns, it is now possible to return to the available data in order to estimate the magnitudes of the various causal effects posited in that model. My focus in this and the two subsequent sections will be on the determinants of expectations and preferences across a variety of campaign settings tapped by the 1980 study. Although the bandwagon effect of expectations on preferences is the causal effect of central substantive interest, my results will also shed some light on the role of political issues and evaluations in the development of pre-convention preferences, and less directly on the role of the media in generating and sustaining bandwagon phenomena.

Parameter estimates reflecting the determinants of expectations and preferences for Democrats in the post-Iowa survey are shown in Table 1. The estimates in the first part of the table correspond to the model on the left side of Figure 1. They indicate that preferences for Carter as the Democratic nominee were significantly influenced by evaluations of Carter and Kennedy on a variety of issues. To facilitate comparisons across issues, all of these evaluations were scaled to range from zero to one.[8] Thus, the relative magnitude of the parameter estimates give a direct indication of the relative importance of different issues in determining nomination preferences. In order to interpret these magnitudes more directly, it is useful to calculate the change in the probability of preferring Carter as the Democratic nominee associated with each of the indicated second-stage logit coefficients. These changes in probability, each calculated for an idealized voter otherwise indifferent between the two candidates, are shown in the last column of Table 1.[9]

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[6] The expectations question reads, "Here is a list of people who are possible choices for the Democratic (Republican) Party nomination for President this year. Who on this list do you think is *most likely* to win the Democratic (Republican) nomination?" (NES V153, V178). The preference question is, "Of all the candidates on this list, who is *your* first choice for the Democratic (Republican) nomination this year?" (NES V176, V209).

[7] The two-stage logit procedure serves here to approximate a more coherent but far more complicated maximum likelihood procedure based on the probit assumption that the underlying continuous distribution of expectations and preferences is bivariate normal. For discussions of the relevant methodological issues see Lee (1981) and Maddala (1983, pp. 242-252). The likely practical effect of the simplification employed here is to underestimate somewhat the standard errors of the second-stage parameter estimates.

[8] The original survey items used to generate the issue evaluations were of three types. Evaluations of Carter's handling of Iran were derived directly from a seven-point response (NES V999). Comparative evaluations of the candidates' abilities to "solve economic problems" and "provide strong leadership" were derived from comparative ratings on four-point items (V663, V664, V690, V691, V744, V745, V789, V790). Comparative evaluations of the candidates' positions on taxes vs. services, defense spending, and (for the Republicans) overall ideology were derived from relative distances of the perceived candidate positions from respondents' own positions on a seven-point scale (V944, V947, V954, V1081, V1082, V1083, V1085, V1089, V1114, V1115, V1116, V1118, V1122). In every case, the resulting issue evaluations were scaled so that zero represents the strongest possible preference for Kennedy, 1.0 represents the strongest possible preference for Carter, and .5 represents neutral or missing values. Thus, all of the issue effects in Table 1 (and subsequent tables) are in the expected direction.

[9] Because the logit model posits nonlinear effects, the actual probability change associated with each variable depends on a respondent's overall probability of preferring Carter. It is customary to evaluate the change in probability for a respondent whose score on each variable equals the sample mean, but this sample-specific rule of thumb makes it impossible to compare probability changes across surveys. Given my interest in discerning an intelligible pattern in the results from four

Table 1. Determinants of Preferences and Expectations for Carter, January-February ($N$=655)

| | Parameter Estimate | Approximate Standard Error | Change in Probability |
|---|---|---|---|
| Preferences[a] | | | |
| Expectation | .375 | .162 | .093 |
| Leadership | 1.543 | .886 | .368 |
| Economy | 3.082 | .753 | .647 |
| Taxes vs. services | 1.906 | .882 | .443 |
| Defense | 1.834 | .865 | .429 |
| Iran | 1.226 | .441 | .297 |
| Intercept | −5.535 | .805 | – |
| Expectations[b] | | | |
| Preference | .661 | .077 | .164 |
| Media exposure | .750 | .471 | .185 |
| Education | .0559 | .0377 | .014 |
| Female | −.340 | .214 | −.085 |
| Black | −.704 | .259 | −.174 |
| Intercept | .778 | .469 | – |

[a] −2 log likelihood = 667.4; 76.8% correctly classified.
[b] −2 log likelihood = 567.9; 78.0% correctly classified.

The estimates in the second part of Table 1 correspond to the model on the right side of Figure 1. They indicate—not unexpectedly, given the political situation at the time of the post-Iowa survey—that highly educated Democrats and those most exposed to the news media were most likely to expect Carter to be the party's nominee. In addition, Carter was more frequently named as the expected nominee by men than by women and by whites than by blacks.[10]

These results are consistent with the idea that expectations are, in part, a reflection of political awareness, at least when the political environment provides reasonably clear and consistent signals about who is likely to win. But expectations are more than just beliefs based on what people are *told* will happen; to some extent, they also reflect what people *hope* will happen. This projection effect is captured by the parameter estimate for preferences in the expectations equation. Respondents who wanted Carter to win the nomination were significantly more likely than others to expect that he would, in fact, be nominated, even after controlling for differences in the social characteristics of these two groups. A one-unit change on the underlying continuum of preference apparently produced a change of more than sixteen percentage points in a respondent's probability of expecting Carter to be nominated. More concretely, an exogenous increase of one percentage point in an undecided respondent's probability of preferring Carter apparently produced an increase of .661 percentage points in her probability of expecting Carter to win the nomination. Preferences thus appear to have been a powerful determinant of expectations during the early part of the nominating process.

By way of comparison, we can return to the first part of Table 1 to assess the reciprocal impact of expectations on preferences. The parameter estimate, .375, suggests that an exogenous increase of one percentage point in an undecided respondent's probability of expecting Carter to be nominated produced an increase of a little more than a third of a percentage point in her probability of preferring Carter as the Democratic nominee. Although only a little more than half as

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separate campaign contexts, it seems preferable here to adopt a less descriptive but more consistent baseline, evaluating the change in probability for a respondent with a .5 probability of preferring Carter. Note, however, that this is precisely the respondent for whom, under the assumptions of the logit model, the change in probability associated with each variable is greatest. Thus, the changes in probability shown in this and subsequent tables should not be interpreted as average effects for each variable in the sample at hand, but as maximum potential effects for respondents at the point of greatest uncertainty.

[10] My measure of media exposure is a scale constructed from survey responses concerning exposure to political conversation, network television news, news magazines, newspapers, and radio news (NES V12, V22, V24, V26, V28). Education is measured by years of formal schooling (V1483). The effects of race and sex, captured by dichotomous variables for blacks and females (V1701, V1702), presumably reflect differences in exposure and attention to (mainstream) political news media which persist after controlling for education.

big as the projection effect, this bandwagon effect is big enough to have very significant political consequences. Elsewhere (Bartels, 1983, pp. 191-193) I have estimated that Carter's victory in the Iowa caucuses in 1980 increased his probability of being nominated by about 12 percentage points, and that a single week's results later in the primary season increased his probability of being nominated by as much as 20 percentage points. If these changes in probability can be translated directly into changes in subjective expectations, the results presented here suggest that Carter's primary and caucus successes may have increased his overall popular support by from four to seven percentage points in a single week.

### Iowa to New Hampshire: Republicans

In many respects, the Republican contest in the month before the New Hampshire primary provides the most interesting political circumstances of the 1980 nominating campaign. George Bush had just scored a startling upset in the Iowa caucuses and was suddenly "the hottest property in American politics" (Cannon & Peterson, 1980, p. 137). Bush himself, in his campaign speeches, referred frequently and gleefully to "Big Mo," and not without reason: in a period of ten days, Bush gained more than 20 points in the polls, surpassing Reagan as the first choice of Republicans for their party's nomination. The first wave of the 1980 CPS National Election Study was perfectly timed to provide a detailed picture of expectations and preferences during this heady month, and thus of a bandwagon almost comparable in magnitude (although not in duration) to Carter's in 1976 and Hart's in 1984.

What were the ingredients of Bush's sudden popularity? The logit estimates in the first half of Table 2 provide some indications. First, it is interesting to note that most of the "issues" included in my analysis had substantially smaller impacts on support for Bush among Republicans than on support for Carter among Democrats. The only exception to this pattern involves the most general (and least substantive) of the "issues"—leadership. Both the pattern and the exception suggest that Bush's support was more diffuse, and less firmly grounded in specific political perceptions, than Carter's. In this respect, the results conform to the stereotypical view of bandwagons like Bush's as being nonideological—even, to some extent, apolitical—in character.

The other interesting difference between Bush's support and Carter's is that expectations had a much greater impact on support for Bush than on support for Carter. On the Democratic side, an exogenous increase of one percentage point in an undecided respondent's probability of expecting Carter to be nominated increased her probability of preferring Carter by about a third of a percentage point. On the Republican side, the corresponding increase in the probability of preferring Bush was more than a full percentage point—a powerful translation of expectations into preferences. It is not hard to understand how bandwagons like Bush's begin to roll when perceptions of success have such a dramatic impact on the preferences of potential voters.

**Table 2. Determinants of Preferences and Expectations for Bush, January-February ($N$=490)**

| | Parameter Estimate | Approximate Standard Error | Change in Probability |
|---|---|---|---|
| Preference[a] | | | |
| Expectation | 1.176 | .203 | .286 |
| Leadership | 1.340 | 1.281 | .323 |
| Economy | –.316 | 1.282 | –.079 |
| Taxes vs. services | –.084 | 1.012 | –.021 |
| Defense | 1.191 | 1.147 | .289 |
| Ideology | .496 | 1.118 | .123 |
| Intercept | –1.607 | 1.113 | – |
| Expectations[b] | | | |
| Preference | .632 | .140 | .157 |
| Media exposure | 1.178 | .611 | .286 |
| Education | –.0549 | .0598 | –.014 |
| Female | –.524 | .236 | –.130 |
| Black | 4.046 | 1.242 | .766 |
| Intercept | .140 | 1.124 | – |

[a] –2 log likelihood = 379.2; 84.1% correctly classified.
[b] –2 log likelihood = 469.8; 78.2% correctly classified.

**Table 3. Determinants of Preferences and Expectations for Carter, April ($N$=620)**

| | Parameter Estimate | Approximate Standard Error | Change in Probability |
|---|---|---|---|
| Preferences[a] | | | |
| Expectation | .087 | .117 | .022 |
| Leadership | 3.754 | .696 | .735 |
| Economy | .462 | .766 | .115 |
| Taxes vs. services | 2.632 | .906 | .577 |
| Defense | 1.963 | .811 | .455 |
| Iran | 2.114 | .408 | .484 |
| Intercept | −5.944 | .693 | – |
| Expectations[b] | | | |
| Preference | .595 | .096 | .148 |
| Media exposure | .927 | .555 | .228 |
| Education | .1314 | .0417 | .033 |
| Female | −.800 | .279 | −.197 |
| Black | −1.194 | .297 | −.290 |
| Intercept | .996 | .548 | – |

[a] −2 log likelihood = 654.5; 72.6% correctly classified.
[b] −2 log likelihood = 407.4; 87.3% correctly classified.

Given the impact of expectations on preferences for Bush in the early phase of the 1980 campaign, it seems reasonable to inquire into the determinants of these expectations. Some evidence is provided by the results in the second half of Table 2. Three points are worthy of notice. First, the projection effect, measured by the parameter estimate for preference, is virtually identical in magnitude to the corresponding projection effects among Democrats in both the early and late primary season surveys. This similarity provides some indication of the persistence of projection as a behavioral phenomenon across a variety of political settings.

Second, media exposure seems to have played a powerful role in the development of expectations that Bush would win the Republican nomination; indeed, the estimated effect of media exposure on Republican expectations is more than 50% greater than the corresponding estimated effect of media exposure on Democratic expectations at the same point in time. It is worth recalling that Bush's objective chances of being nominated were, despite his success in Iowa, certainly no better than Carter's at this stage in the campaign. Thus, the greater impact of media exposure on expectations on the Republican side is a reflection not simply of a less ambiguous political reality, but also of the general emphasis and specific interpretations placed on that reality by the media to which potential voters were more or less heavily exposed.

A third, related, point is that education appears to have had a *negative* effect on Republicans' propensity to perceive Bush as the most likely nominee in the month after Iowa. This is the only instance, of the four investigated here, in which media exposure and education had counteracting rather than reinforcing effects. Although the parameter estimate for education is too imprecise to provide definitive evidence, it is tempting to speculate that, in this instance, the kind of media exposure instrumental in increasing respondents' perceptions of Bush's chances was primarily *uncritical* exposure to the media's month-long fascination with Bush, his campaign, and his "Big Mo" in the wake of his Iowa victory.

## Bandwagon and Projection Effects Later in the Primary Season

In order to test the persistence of the relationships linking expectations and preferences in nominating campaigns, I have replicated the analyses of the post-Iowa survey described above, using the separate cross-sectional survey conducted in April 1980. By that time many of the primaries and caucuses were over, and there was considerably less objective uncertainty than there had been in February that Carter and Reagan would be their parties' nominees. Given these changes in the political situation, one might expect either the bandwagon or projection effects, or both, to be less powerful in April than in February.

The results of the replication, shown in Tables 3 and 4, are in keeping with the changes in the political situation in the intervening months. On

**Table 4. Determinants of Preferences and Expectations for Reagan, April ($N$=439)**

| | Parameter Estimate | Approximate Standard Error | Change in Probability |
|---|---|---|---|
| Preferences[a] | | | |
| Expectation | .007 | .146 | .002 |
| Leadership | .868 | 1.137 | .214 |
| Economy | 2.136 | 1.067 | .488 |
| Taxes vs. services | 1.019 | .875 | .249 |
| Defense | 2.572 | .906 | .567 |
| Ideology | 1.888 | .816 | .440 |
| Intercept | −4.908 | .798 | – |
| Expectations[b] | | | |
| Preference | .801 | .196 | .198 |
| Media exposure | 1.841 | .568 | .430 |
| Education | .1174 | .0504 | .029 |
| Female | −.565 | .292 | −.140 |
| Black | −.644 | .522 | −.160 |
| Intercept | .183 | .669 | – |

[a] −2 log likelihood = 535.5; 67.9% correctly classified.
[b] −2 log likelihood = 335.7; 85.2% correctly classified.

the Democratic side (Table 3), evaluations of Carter's handling of the Iranian crisis and more general evaluations of the candidates' relative "leadership" qualities gained substantially in importance as determinants of nomination preferences as the crisis wore on from February to April. At the same time, perhaps because the rate of inflation began to recede from its January peak of 20% and public attention began to turn from the economy to foreign policy, the effect of comparative economic evaluations on nomination preferences declined significantly. Finally, the bandwagon effect almost disappeared, with an exogenous increase of one percentage point in an undecided respondent's probability of naming Carter as the most likely nominee adding less than one-tenth of a percentage point to her probability of preferring him herself.

On the Republican side (Table 4), comparative evaluations of the candidates with respect to the economy, defense, taxes vs. services, and ideology all increased substantially in importance as determinants of candidate preferences.[11] The only comparative evaluation to decline in importance between the two surveys was the one involving leadership. In addition, for Republicans as for Democrats, the bandwagon effect so evident before New Hampshire had virtually disappeared by April. By this point in the campaign, apparently, respondents in both parties were relying more heavily on substantive political perceptions—and much less heavily on strategic considerations or momentum—than in the earliest weeks of the campaign.

The determinants of expectations in the second part of Tables 3 and 4 also reflect changes in the political situation between February and April. In each party, as it became more and more obvious that Carter and Reagan would be nominated, media exposure and political awareness became more powerful determinants of respondents' expectations. Among Republicans, the contrasting effects of exposure and education in the month after Iowa had become congruent by April, suggesting that the media's interpretation of the race may have changed sufficiently to make sophisticated respondents more, rather than less, likely to accept it. However, the increasing importance of objective determinants of expectations did little to reduce the importance of projection; even in April, with both races for all practical purposes decided, from 60 to 80% of any exogenous change in preferences was projected onto expectations about who would be nominated. This result provides further evidence of the resilience of projection as a psychological mechanism, even in situations where objective cues about the shape of political reality are quite powerful.

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[11] By April, Bush was so far out of the race (only 3.4% of the Republican and Independent respondents still expected him to be nominated) that Reagan had clearly become the sole focus of the Republican campaign. For this reason, I analyzed preferences and expectations for Reagan, rather than for Bush, in the April survey. In order to confirm that the comparisons between the two periods made in the text do not depend crucially on which candidate is the focus of analysis, I also analyzed preferences and expectations for Reagan in the earlier survey; the results were consistent with those reported above.

### Patterns and Implications

Having analyzed the determinants of expectations and preferences in a variety of campaign settings, I turn in this section to a discussion of how my results fit together, and of what they suggest about the nature and role of momentum in the presidential nominating process.

From the political standpoint, the candidate preferences of potential primary voters are obviously of key importance, and my analyses shed some light on the determinants of these candidate preferences. In almost every instance, comparative evaluations of the candidates on the issues (the economy, taxes and services, defense, the Iranian crisis) had strong effects on potential voters' preferences. These effects would not be surprising, were it not for the occasional claims of campaign observers (e.g., Gopoian, 1982) that issues play only a minor role in primary voting. The evidence presented here indicates that issue perceptions do matter, particularly in stable contests involving well-known candidates.

What about less stable contests involving less well-known candidates? This case is the more interesting one if our goal is to understand the role of momentum in the nominating process. My analysis of support for Bush at the apogee of his challenge provides a fairly clear picture of the distinctive political character of bandwagon phenomena. If I were to construct a typical profile of bandwagon support, it would include three major elements: unusually weak effects of specific issue perceptions, relatively strong effects of more diffuse perceptions involving general qualities like leadership, and very strong positive effects of expectations on preferences. Thus, it appears that supporters flock to the candidate with momentum mostly because he is new, exciting, and getting a lot of attention, and that they bolster this diffuse support with more specific, reasoned political judgments only later (or, if the candidate fades, not at all).

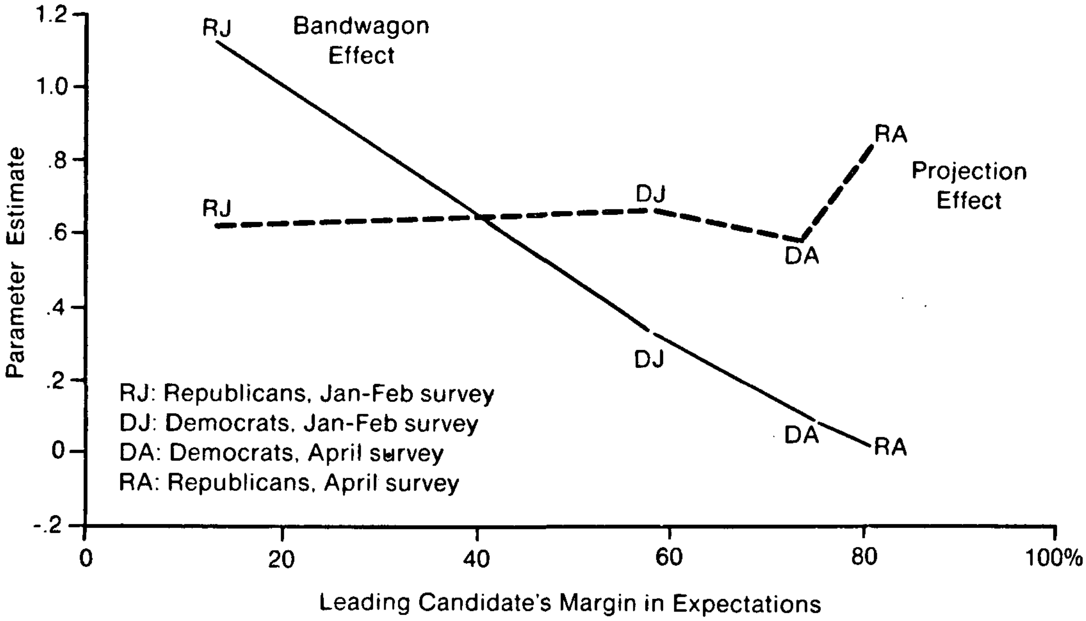
Under what kinds of political circumstances do these bandwagon phenomena occur? Recent experience suggests that they are most common when unexpected successes by little-known candidates disturb the stability of a nominating campaign; certainly, Carter in 1976, Bush in 1980, and Hart in 1984 all fit comfortably within this general pattern. In principle, we could construct a continuum of political contexts defined by several related features of nominating campaigns, including the number and political stature of the competing candidates, the number of primaries and caucuses that have already occurred, and the apparent closeness of the race. It is not completely clear how these contextual factors fit together in determining the importance of momentum. Fortunately, they tend to covary in actual campaigns: as the primary season progresses the field is winnowed, surviving candidates tend to become better known, and expectations about who will be nominated tend to become less problematic and more widely shared. Thus, it is possible to characterize different campaign situations fairly straightforwardly on the basis of simple indicators, without doing gross violence to the underlying political reality.

For example, it is possible to measure the preponderance of expectations for the frontrunner in each of several different campaign situations. The greater the frontrunner's margin in perceived chances of being nominated, the less volatile the situation, and presumably the less closely that situation approximates the bandwagon conditions typified by the emergence of Carter, Bush, and Hart. When applied to the four separate campaign situations tapped by the 1980 data (Democrats between Iowa and New Hampshire, Republicans between Iowa and New Hampshire, Democrats in April, Republicans in April), this simple criterion successfully distinguishes the situation prevailing in April, when both parties' nominations were practically locked up, from the more fluid situations prevailing in January and February. In addition, it successfully distinguishes the volatile Republican situation after Iowa, when Bush was rapidly vaulting into public prominence and into a narrow lead in the polls, from the much less volatile situation on the Democratic side at the same time, when both candidates were already very well known and their relative standings in the campaign already seemed fairly stable.

Once we begin to distinguish campaign situations in this way, it is possible to explore systematic variations in observed political behavior across different political contexts. In the present case, the most interesting such variation is in the importance of the bandwagon effect from one setting to another. I have already indicated above that the magnitude of this effect varied substantially from the early Republican campaign (where it was very large and positive) to the April campaigns in both parties (where it was essentially zero). In Figure 3, this variation is graphically related to the volatility of each campaign situation, measured by the leading candidate's margin in expectations of being nominated. The relationship is strong and clear: the bandwagon effect is most important in settings where the nomination is perceived to be very much in doubt, and least important in settings where one candidate has established a clear predominance.

By contrast, it is clear from Figure 3 that the magnitude of the projection effect of preferences on expectations is much less variable. In one instance, among Republicans in April, the estimated

**Figure 3. Variation Across Campaigns in Bandwagon and Projection Effects**



projection effect is noticeably larger than in the other three instances; but this difference does not correspond to any obvious political feature of the campaign and may simply reflect the stochastic nature of parameter estimates. The most reasonable interpretation of the general pattern of these results is that projection is a quite persistent behavioral phenomenon, varying little in impact across the range of campaign settings represented in the 1980 data.

The persistence of the projection effect across a range of campaign settings may make it more or less interesting as a topic for detailed analysis. But in either case, the danger involved in ignoring the phenomenon of projection is easily illustrated by comparing the estimated effects of expectations on preferences presented above with similar estimates based on a simpler model in which projection plays no role. In this simpler model, preferences are determined by issue evaluations and expectations, as before, but expectations are treated as exogenous. The results of the comparison, shown for Democrats and Republicans in the pre-New Hampshire survey in Table 5, are striking. In each case, the effect of expectations on preferences is grossly overestimated by the simpler model—on the Republican side by a factor of two, and on the Democratic side by a factor of seven. The problem in each case is that the portion of the correlation between expectations and preferences actually due to projection is wrongly attributed to the bandwagon phenomenon in the simpler, misspecified model. In the presence of substantial projection effects, models that do not allow for reciprocal causation simply cannot be trusted to provide meaningful results.[12]

Given a more realistic model that takes due account of projection, it does seem possible to estimate the reciprocal influence of expectations on preferences in a meaningful way. The results presented here suggest that this bandwagon effect can play a major role in the dynamics of nominating campaigns. Under circumstances like those prevailing in the Republican party in early 1980 (and in the Democratic party in early 1984), expectations appear to be translated very powerfully into preferences, making it possible for candidates with momentum to generate new support at a prodigious rate.

[12] A similar problem presumably plagues the analysis of "electability" as a determinant of preferences among party activists offered by Stone & Abramowitz (1983). Although one of their models does treat electability as an endogenous variable, Stone & Abramowitz ignored the possibility that perceptions of electability may be determined partly by personal preferences. To the extent that such a projection effect exists, an analysis like Stone & Abramowitz's will tend to overestimate the importance of electability as a determinant of preferences.

**Table 5. Estimated Bandwagon Effects with and without Projection (January-February)**

| Effect of Expectations on Preferences among | With Projection | Without Projection (Misspecified Model) |
|---|---|---|
| Republicans | 1.176<br>(.203) | 2.535<br>(.285) |
| Democrats | .375<br>(.162) | 2.571<br>(.364) |

My analysis also provides some suggestive evidence about the nature of bandwagon support and about the mechanisms by which it is generated. Presumably, once some political event triggers the process, the behavior of the news media provides a critical link between political reality and the perceptions of potential voters. Horse race coverage, polls, and projections provide a steady barrage of information about who is winning and who is gaining ground.[13] The impact of media exposure on expectations reflects the results of this barrage. More important, the relative diffuseness and lack of substantive political grounding of bandwagon support reflects what the media do *not* provide with nearly as much volume or enthusiasm at this stage in the campaign: specific information about the candidates' qualifications to be president.

Finally, my analysis provides some indication of the limits of the bandwagon phenomenon as an explanatory variable. Most of the formal models of the dynamics of the nominating process (Aldrich, 1980b; Bartels, 1983, chap. 4; Brady, 1984, pp. 49-55) suggest that expectations and preferences should drive each other in a continuing upward spiral, with successful candidates doing progressively better and others falling by the wayside as the campaign proceeds. But the three most notable bandwagons in recent nominating campaigns—Carter's in 1976, Bush's in 1980, and Hart's in 1984—all appeared to slow during the course of the primary season. The results presented here reflect a similar slowing at the individual behavioral level: although bandwagons were much in evidence immediately after the Iowa caucuses, particularly on the Republican side, they had all but disappeared by April.

Only further investigation will make clear whether these results indicate a more general pattern. But if they do, they put the dynamics of the presidential nominating process in a new light. The bandwagon phenomenon focused on by earlier analysts, important as it obviously is, may not necessarily lead in the real world to the kinds of outcomes predicted by our simple models. Rather than doing better and better (or worse and worse) in an unbroken cycle, candidates may reach plateaus of support determined in part by their political skills and circumstances.[14] Why? One reason may be that the media, the public, or both tend to tire of the horse race and, eventually, turn their attention to less volatile, more substantive considerations (or simply stop paying attention to the campaign altogether).

The dynamic properties of such a process (and their political implications) would depend crucially on a variety of behavioral and institutional parameters. It might be that momentum stops mattering only after one candidate is so far ahead that the race is, for practical purposes, over. In that case there would be little comfort in the fact that attention turns from the horse race before the horses are back in the stables. On the other hand, and more benignly, momentum might shape the nominating process not by propelling one candidate willy-nilly to the nomination, but by creating a framework in which genuine intraparty competition takes place, winnowing out many of the contenders, giving others unforeseen prominence, but eventually confronting party voters with a meaningful substantive choice.

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[13] For a useful description of the content of media coverage during the 1980 nominating campaign, see Robinson, Conover, & Sheehan (1980). Also relevant is the earlier analysis by Matthews (1976).

[14] Starting from somewhat different substantive considerations, Brady (1984, pp. 39-49) constructed a mathematical model of the nominating process exhibiting dynamic properties consistent with this outcome.

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# Communications

## ON ARTICLES

### Comment on The Problem of Strategic Voting under Approval Voting (Vol. 78, December 1984, pp. 952-958)

Niemi (1984) contains egregious errors and radically biased misinterpretations. We begin with two errors, one of omission and one involving an internal contradiction that is unrelated to approval voting.

1. Niemi cites a number of results for voters with dichotomous preferences from our first article on approval voting (AV) (Brams & Fishburn, 1978), claiming that "all of these results depend on the existence of dichotomous preferences, a contrived and empirically unlikely assumption." This is not true. In a subsequent article (Fishburn & Brams, 1981) and in our book (Brams & Fishburn, 1983, chap. 3), we showed that the election of a Condorcet candidate does not depend on all voters' having dichotomous preferences. Indeed, other readily interpretable sufficient conditions *guarantee* the election of a Condorcet candidate when voter preferences are not dichotomous.

For example, we proved that whenever any other common nonranking voting system (e.g., plurality voting [PV] or PV with a runoff) guarantees the election of a Condorcet candidate when voters use admissible (i.e., undominated) strategies, so does AV, but not vice versa. Thus, AV dominates these other systems with respect to a Condorcet "guarantee criterion." If a candidate is not guaranteed election when voters use admissible strategies, computer simulations by Bordley (1983) and Merrill (1984, in press) show that approval voting, on the average, does very well in electing Condorcet candidates, compared with ranking as well as nonranking systems. We give several empirical examples in our book that support this theoretical result (Brams & Fishburn, 1983), as do Merrill (1981), De Maio and Muzzio (1981), De Maio, Muzzio, and Sharrard (1983), Nagel (1984), and Chamberlin, Cohen, and Coombs (1984).

To be sure, the assumption of dichotomous preferences is the only necessary and sufficient condition for the assured election of Condorcet candidates when voters use admissible strategies. But to ignore our copious results on the election of Condorcet candidates under AV as well as other voting systems is to leave the reader with the false impression that we take into account only "a contrived and empirically unlikely" case. This seems to us a serious error of omission.

In fact, we have empirical evidence that indicates dichotomous preferences are rather common, especially in three-candidate elections. However, even if dichotomous preferences were uncommon, we emphasize that many of our results are not limited to the dichotomous case.

2. Result 12 involves an internal contradiction: Niemi has voters designate some candidates as "approved," the others as "disapproved," and then shows that sophisticated approval voting may entail voting for some disapproved candidates. But this apparent contradiction occurs simply because the goal of sophisticated voting—to obtain preferred outcomes through the successive elimination of dominated strategies—is inconsistent with imposing constraints on the choice of certain strategies (i.e., those that involve voting for disapproved candidates).

Emphatically, this contradiction has absolutely nothing to do with approval voting. Designating only voters' first choices under PV as "acceptable" (to use a different word from "approved"), for example, leads to the same contradiction: it may be optimal for some sophisticated voters under PV to vote for unacceptable candidates.

Thus, the contradiction is internal in the sense that the constraint is incompatible with strategic behavior. It is not peculiar to approval voting but holds generally for voting systems. To avoid such a contradiction, but still retain the notion that only certain candidates are considered "approved" or "acceptable" by some voters, one could assume these constraints of voters are common knowledge (among all voters) in a game of complete information, which is the usual informational assumption made about voter preferences under sophisticated voting. Then sophisticated calculations could be made, given knowledge of these constraints and the presumption that they will be adhered to, and involve no internal contradiction.

We turn now to three significant misinterpretations of our results.

1. The claim of Result 1 is that "AV almost begs the voter to think and behave strategically." Speaking normatively, it is not clear to us why voters should *not* have to consider their choices carefully before they vote, especially when these are sincere. But quite apart from this issue, we were careful to distinguish in our original article

(Brams & Fishburn, 1978) "sincerity" from "strategyproofness" (when a voter has a unique sincere, i.e., dominant, strategy) precisely to indicate that sincere strategies are not necessarily strategyproof.

The important point, though, is not that AV is vulnerable to strategic voting—virtually all systems are—but how it compares to other systems with respect to encouraging an "honest expression" of voter preferences. We claim it is more sincere and strategyproof than other nonranking systems; perhaps Niemi has in mind other criteria that make AV appear less favorable in promoting an honest expression of preferences. It would be helpful to know what these are so that general comparisons could be made with other voting systems.

2. Results 2-5 for sincere AV are correct, but the impression the reader is left with is highly misleading. If one substitutes PV for AV in all these results, plus Result 1, the statements would still be true.

Moreover, if one drops the phrase, "In the absence of dichotomous preferences," for Results 2-5 (Result 6 does not include this qualification), the statements would also be true for PV. In other words, AV is not only *not* worse than PV in all these situations, but it is in fact better because these alleged "undesirable features" do not hold for AV—but do for PV—in the dichotomous as well as nondichotomous cases.

3. As with the earlier results, Results 7-11 for sophisticated AV (except for Result 8, which is inapplicable to PV) obtain for sophisticated PV without the qualifying phrase, "In the absence of dichotomous preferences." (Result 12 also includes this qualification, but as we pointed out earlier, it contains an internal contradiction not tied to AV.) Hence, at least as far as comparisons with PV are concerned, AV is *uniformly superior* with respect to Niemi's so-called problems.

This conclusion is supported by new studies, published or completed since our book appeared, including the computer simulation results for ranking as well as nonranking systems cited earlier (Bordley, 1983; Merrill, 1984, in press), relative-efficiency results for sincere voting (Hoffman, 1983), and a median-voter Nash equilibrium result, when there are more than two candidates, that does not hold for PV (Cox, 1985). This latter result is game-theoretic, and thus in the spirit of sophisticated voting, although Nash equilibria are more robust than sophisticated equilibria because they are not dependent on the assumption that all other voters act sophisticatedly by eliminating dominated strategies.

**Conclusion.** We do not mean to imply by our response that AV is perfect—no voting system is—but rather that it has, in our opinion, far more attractive properties than any other practical system of which we are aware. It is unfortunate that Niemi chose to attack this system without considering how other systems, on a comparative basis, do on his own dozen or so "negative" criteria, as well as on more positive criteria—including "simplicity" and "easiness"—that have been proposed (e.g., see Nurmi, 1983).

We have no doubt that the debate about AV will continue—as indeed it should, because this election reform represents a radical departure from traditional ways of aggregating individual preferences into social choices. For the interested reader, this debate takes on a more empirical flavor in the exchange between Arrington and Brenner (1984) and Brams & Fishburn (1984).

STEVEN J. BRAMS

*New York University*

PETER C. FISHBURN

*AT&T Bell Laboratories*

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## Reply to Brams and Fishburn

Let me first treat what Brams and Fishburn regard as my errors.

1. Brams and Fishburn think I leave a "false impression" of approval voting. That is ironic, because one of the reasons I wrote the article is to counteract the false impression they have created of approval voting. Although Brams and Fishburn admit that it is not perfect, the general impression one gets from their work is that approval voting contains only virtues and no faults. If I have gotten across the point that approval voting—like other voting systems—has flaws, I will have accomplished one of my main goals.

2. With respect to my result 12, I am quite happy to agree that the constraint I imposed (approving only one's first choice) is incompatible with strategic behavior. I am happy to agree, that is, as long as the reader realizes that *if* an individual approves only his first choice (but can order the other two choices), then a) sophisticated approval voting need not select a Condorcet candidate (my result 7); and b) the individual may have to accept a less preferred alternative than if he behaved strategically.

Now I turn to what they call misinterpretations.

1. In the second paragraph under this point, Brams and Fishburn again apply the reasoning that I think is so misleading. In simplest form, the problem is this. They ask, does approval voting encourage an "honest expression" of voter preferences? Their answer: Approval voting encourages sincere behavior. But that response does not answer the question they asked. I grant that approval voting is more frequently sincere in the technical sense. But sincerity as used with approval voting is not equivalent to honesty. (Or, if you like, one might say that it is equivalent to a very limited form of honesty.) The point is that under approval voting one can be strategic and at the same time be (technically) sincere. Thus, for example, you can calculate whether you are more likely to get what you want by voting for *a* or for *ab*—even if you much prefer *a*—but you are regarded as still behaving sincerely (if your preference order is *abc*). So will approval voting encourage more honest expressions of preferences? I don't think anyone can really be sure. I have shown that under approval voting (and plurality voting) voters do not simply express preferences irrespective of political circumstances (Niemi & Bartels, 1984). Those results suggest that approval voting may not encourage honesty any more than plurality voting, but, of course, they do not fully answer the question. In any event, my main point in the article was to stress that the greater sincerity of approval voting does not answer the question about how honestly voters express their preferences.

2. I agree that all of what I said applies to plurality voting as well. My point is simply the reverse. Do not forget that most things that affect plurality voting also affect approval voting.

3. Comparisons of voting systems are useful, but simulations do not answer all of our questions. Simulations often assume random distributions of individuals, no cooperation whatsoever, and sometimes optimal voting strategies. Although valuable—and I have done them myself—they do not tell us how people would actually vote. Nor do they tell us whether or not people will be happy with the outcome and think that the process leading to the outcome was a fair one.

Let me conclude with a short story that says something (to me at least) about the way people might react to approval voting. It involves taxes and schools, something about which individuals have very strong opinions. For this reason, I think that it is a good test of how people would react to an approval voting situation and whether or not they would be happy with an approval voting outcome.

Our local school board has outlined five proposals for dealing with the problem of an aging school: 1. Only state-mandated repairs, such as removal of asbestos; 2. Renovations beyond those state-mandated, but no "moving of walls"; 3. Major renovations, including gutting portions of the school; 4. A new school for grades 9-12; 5. A new school for grades 7-12. A chemist on our faculty (having seen the review of *Approval Voting* in *Science*) called me and suggested that were a vote to be taken on these five alternatives, it should be by approval rather than plurality voting. His concern was that if there were a plurality vote, all of those concerned primarily with taxes would vote for alternative 1, whereas the "pro-school" advocates would split their votes among proposals 2-5. Under approval voting, the pro-school group could vote for more than one alternative, and presumably one of the last four proposals would beat the minimal-repair alternative.

If faced with such a vote, however, how would the pro-school faction in fact behave? Suppose you most favored building a new school for grades 9-12? Should you vote for both new school projects in order to ensure a new building even though you do not really want seventh and eighth graders going to school with ninth through twelfth graders? Should you vote for the major repair option as well since you are convinced that current facilities are inadequate? Should you even vote for the minor repairs option in order to avoid the state-mandated-only alternative from winning? Or should you only vote for your first choice lest you do yourself in by also giving votes to alternatives you are less happy with?

When the voting is completed, would you be happy with the results? Suppose the new 9-12 school lost by a narrow margin to the major repairs or the 7-12 option? If you (and presumably some others) voted for one of those alternatives as well as for the 9-12 school, was the winning alternative really the most preferred? (In part, how you would feel would probably depend on what information was provided about the vote—e.g., vote totals only, approval votes themselves, or preference order data—a topic I have written about elsewhere; Niemi, 1984.)

Would it be better to hold a plurality vote among the five alternatives? Probably not. However, given the known problems of plurality voting, it is not likely that such vote would actually be taken. Rather, the alternatives would be discussed at great length, with some compromises attempted. Then, a single alternative would probably be proposed for a vote by the district. Not a perfect solution, of course, but one that at least involves a great deal of input before the vote. The danger is that advocates of approval voting will convey the impression that their method so smoothly solves the problems inherent in conflict situations that decisionmakers will mistakenly avoid the process just outlined in favor of an approval vote.

So that Brams and Fishburn do not misunderstand me again, I hasten to add that they have nowhere said that approval voting solves all multialternative conflicts. But the impression one gets from their strong advocacy is that it will solve all or almost all such problems. The possibility that it will erode the give and take of politics before a vote—along with the difficulties of knowing whom to vote for and whether to be satisfied after the vote—are some of the major problems involved in approval voting.

**Postscript.** Footnote 9 in my article should be deleted. Dan Felsenthal and Zeev Maoz of the University of Haifa found counterexamples of what Gretlein and I thought was a general result. Nothing else in the article is affected.

RICHARD G. NIEMI

*University of Rochester*

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## Rejoinder to Niemi

We respond to Niemi's "short story," which is quite independent of our critical remarks, with a very short comment. In the innumerable situations in which protagonists are unwilling to compromise their differences, or candidates refuse to bow out of elections gracefully, are we to deplore their behavior as simply bad form? For better or worse, this is the "politics" that Niemi wants to preserve. We want to preserve the ability of voters to cope with precisely those situations in which politicians do not practice self-denial and, for the convenience of voters, reduce voter choices to easy binary ones.

STEVEN J. BRAMS

*New York University*

PETER C. FISHBURN

*AT&T Bell Laboratories*

## Comment on Wilson (Vol. 77, December 1983, pp. 895-910)

"To test the extent of corporatist interest group politics in France" (p. 896), Frank L. Wilson presents and analyzes data derived from interviews regarding the frequency and perceived efficacy of various forms of interest group activity vis-à-vis the state. The data are interesting in many respects, but it is difficult not to be troubled by the manner with which Wilson handles their presentation and analysis. To begin with the former, the data are presented in a methodologically curious fashion which serves to understate the frequency and perceived efficacy of formal, corporatist-style interactions between recognized/authorized interest groups and the state. The findings reported in Tables 3-6 include the results of interviews not only with the principal officially recognized groups, but also with what Wilson terms "small dissident groups regarded as 'unrepresentative' by the government" (p. 900). Wilson acknowledges in a footnote that "20.4% of those interviewed reported that their groups were not members of the Economic and Social Council" (p. 898), and one can safely infer that many if not all of the groups excluded from the ESC were also excluded from many important committees and were denied formal access to government officials as well; indeed, he notes in another passage (p. 900) that some of the groups in his sample held seats on no committees at all. Needless to say, the inclusion of data related to these unrecognized groups must have decreased the percentages of interviewees reporting that corporatist-style interactions with the state are frequent or effective. One can only wonder why Wilson did not disaggregate his data, separating those for the recognized groups (the obvious focus for a test of the extent of corporatization) from those related to the unrecognized groups, so as to gauge accurately the degree to which the recognized groups rely on and value corporatist-style interactions with the state and to demonstrate the manner in which the behavior of those groups differs from that of the "unofficial" groups.

Even though the percentages of Tables 3-6 are skewed in the manner discussed above, they seem to provide evidence of a significant degree of corporatization in terms of the central criteria listed in Wilson's theoretical framework. He states that one of the "most prominent features" of a neocorporatist pattern of interest group politics "is a set of statutory institutions to accommodate direct contacts between government officials and representatives of authorized interest groups" (p. 896). In a corporatist system, one thus expects to find "well developed, frequent" institutionalized group-state contacts, whereas "under pluralism, statutory contacts between interest groups and government officials are less frequent or nonexistent" (p. 897). How well developed and frequent are such contacts in the French case? Wilson notes that the French system features an "unbelievable quantity" (p. 900) of committees/councils, and he reports that "the form of interest group action most listed as undertaken 'often'" by the group representatives in his sample "was a method linked with corporatist forms of interest representation: participation in the activities of government committees" (p. 898). It strikes me as difficult if not impossible to square these statements with Wilson's conclusion that his "survey of group action in France suggests that the predominant pattern is pluralist" since the "most frequent activities . . . were formal and informal contacts between separate interest groups and political or administrative officials" (p. 907). Somewhere between pages 898 and 907, frequent participation by recognized groups in government committees has been transmogrified from a definitive feature of a corporatist system to evidence of the predominance of pluralism; this system of "interest group pluralism" does indeed seem to have its "peculiarities" (p. 907).

A careful reading of the article enables one to see how Wilson resolves, at least to his own satisfaction, this apparent contradiction: A system of interest group politics can be categorized as essentially pluralist, despite the presence of a profusion of statutory bodies which are the principal focus of activity for a set of privileged groups, if most group leaders rate "the multipartite committees, clearly corporatist in form, as *ineffective*" (p. 907, emphasis added). "The presence of corporatist-type structures does not mean the prevalence of corporatist patterns of interest group politics when governments *fail to give any powers* to these bodies . . . neither conservative nor leftist governments have shown any inclination to grant *real power* to the various committees they have proliferated" (p. 907, emphasis added). No French government, Wilson alleges, has been "disposed to grant *any of its authority*" to interest groups; the state has always, he repeats, "*refused to give these bodies decision-making or other powers*" (pp. 908-909, emphasis added).

But is it true that the leaders of the recognized interest groups all view participation in government committees as ineffective? Is it true that governments have granted no decision-making power or authority to any of the committees? These would seem to be absolutely crucial questions, and one would expect to find answers to them in an article that purports to rely on hard evidence rather than mere impressions. Unfortunately, Wilson does not answer these questions satisfactorily. Nowhere does he present a table showing how the various group leaders rate the significance of participation in these committees on a scale from "very ineffective" to "very effective." The only table that comes close to addressing this issue is Table 5, which reports that only 1.1% of the interviewees rated "participation in government committees" as the "*most* effective means of action" (p. 899, emphasis added). The findings of this table are reinforced by selected quotations on pages 900-901, virtually all of which downplay the importance of institutionalized contacts. For several reasons, however, the evidence cited fails to clinch Wilson's argument that the committees are almost universally viewed as ineffective. First of all, he notes that "69% of [farm representatives] rated committees as rather effective or very effective and not one farm spokesman said that the committees had no or very little policy impact" (p. 901). This passage is quite intriguing, because it seems to undermine the utility of Table 5; after all, the farmers viewed their committees as effective *despite the fact that not a single farm leader rated committee participation as the "most effective" means of action.* The clear implication is that a significant if smaller percentage of leaders from the other sectors may have rated committee participation as at least rather effective. Second, Wilson (1983) fails to cite some relevant data that he presented in another piece. It strikes me that his categorical dismissal of the committees is inconsistent with his assertions (in Wilson, 1982, p. 192) that 82.3% of the group leaders in his sample were usually or sometimes "consulted . . . in advance on legislation affecting their members," that "of those consulted, most felt that the government was usually sincere rather than formal in seeking their opinions on legislative proposals," and that 76% of the interviewees "claimed to be very effective [or somewhat effective] in influencing government policy." The incorporation of these findings into Wilson (1983), it seems logical to assume, would have forced Wilson to draw a more measured conclusion. Third, leaving data aside for the moment, one must question whether the major interest group leaders of France—who can be assumed to be at least minimally rational actors—would devote so much of their limited time and resources to committee participation if they perceived such activity to be totally ineffective.

Although many of the statutory bodies may indeed be a source more of frustration than influence for interest groups, given that officials sometimes do ignore group inputs, Wilson's contention that no French government has "shown *any* inclination to grant real power to the various committees" (p. 907, emphasis added) is insupportable. In Ehrmann's words, "Many times the authoritative decision is made entirely on the basis of the group's suggestions, so that in effect administrative functions are parceled out to socioeconomic forces" (Ehrmann, 1983, p. 205). In fact, some respected observers have argued that the committees constitute a "threatening phenomenon," as recognized groups have "take[n] over administrative functions, and assume[d] . . . certain prerogatives of the State" (Lavau, 1958, p. 82; see also Kuisel, 1981, p. 259). The powers devolved by Center-Right governments to the co-management bodies of the agricultural sector were so significant that the first Minister of Agriculture of the Mitterrand era felt moved to declare: "It is necessary to end the confusion between the role of professional organizations and that of the state. The former must negotiate and contest if they feel it necessary; the state must make the decisions" (Keeler, 1985a). Along similar lines, the "power of decision" regarding commercial development which the Royer Law of 1973 accorded to commissions staffed in large part by representatives of shopkeeper's organizations was significant enough to be denounced by some as an abdication of state power and responsibility (Keeler, 1984b). In addition, the degree of power that conservative governments enabled representatives of big business to wield, through privileged committee posts as well as formal and informal contacts, was sufficient to inspire a respected observer to remark that it amounted to a "dilution" of "the once-sacred distinction between the private and public sectors in France" (Suleiman, 1978, p. 230). None of this evidence, which seems to contradict flatly the argument that the "aloof" French state has never taken the statutory bodies seriously, is dealt with by Wilson.

In his analysis of the significance of French statutory bodies, as well as other aspects of group-state relations which limitations of space prevent me from discussing here, Wilson exaggerates the extent to which France should be viewed as situated near the pluralist end of the pluralism-corporatism continuum. As many interest group theorists have argued in recent years, it is necessary to disaggregate our analyses and acknowledge that "different interest sectors . . . may be organized and relate to the state in quite different ways" (Schmitter, 1977, p. 14). Most available evidence, including Wilson's data, is consistent not with the conclusion that "France seems to be an exception to the purported trend toward corporatism" (p. 908), but rather with a conclusion that interest group politics have been less fully and evenly corporatized in France than in many other West European polities. Corporatization has been limited in the labor sector by both political and organizational factors, but corporatist group-state relations have developed strongly in the agricultural sector and at least moderately in the business case.

JOHN T. S. KEELER

*University of Washington*

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## Reply to Keeler

After accumulating social science data there is then the difficult task of deciding how to present that data in a concise yet thorough fashion. Inevitably, one cannot present all the figures nor disaggregate them in all the ways one would like to do. One must try to minimize distortion and bias in making the selection of the data and of the ways in which to present them. Then comes the equally subjective task of analyzing and interpreting the data.

In his comment, Keeler suggests that I may have presented my data in such a way as to skew the results in order to support my contention that there is little or no corporatist interest group-government interaction in France. Specifically, he complains about my failure to eliminate those respondents whose groups do not belong to any statutory government committee or council in calculating Tables 3-6. My sample, however, was designed to include a representative cross-section of all groups trying to influence politics, whether or not they were deemed representative by the government. To have excluded the responses of respondents from groups not on government consultative committees would have distorted the overall pattern of group activity. In fact, the number of respondents whose groups did not sit on government panels amounted to only 10.1% of the sample, and their exclusion or inclusion would not have affected the pattern presented in my tables.

Keeler contends that my presentation of the data understates the group representatives' assessment of the efficacy of committees in the policy process. My own feeling from the open-ended as well as closed questions is that even those groups heavily involved in government committees are very reserved in the judgments of the effectiveness of these corporatist bodies. Table 1 presents some data not in my article and is drawn from a question asked only those respondents whose groups did take part in official committees. With the exception of farm spokesmen, it indicates the diverse and unenthusiastic reactions of my respondents to committees as a means of influencing policy.

Keeler then goes on to argue that I misinterpret other data in order to downplay the extent of corporatism in France. He points to some of my statistics that he believes demonstrate corporatism: the large number of official committees, the frequency with which my respondents claimed to participate in such committees, and finally the group leaders' perceptions of their overall effectiveness in influencing policy. But corporatism requires more than simply the presence of such bodies. They must in fact determine policy and, with the exception of farm leaders, virtually none of the group representatives claimed that the committees did that.

The exception of agriculture deserves some elaboration. The quantitative sections of my article showing the farm leaders' positive views of their action in committees were based on interviews in 1979. However, as I note in the article, when followup interviews were done in 1982, farm representatives claimed that these committees had no policy impact. Interviews in 1984 confirmed the virtual dismantlement of whatever corporatist relationship may have existed between farm groups and the conservative government under the Socialists. The "corporatist" bodies still exist, and they continue to meet although not as regularly as before. But as one senior farm leader told me in 1984, they are very formal and no longer have much influence on policy. This is

**Table 1. Judgments on the Effectiveness of Committees (%)**

| | All | Business | Farm | Union |
|---|---|---|---|---|
| Quite effective | 23.0 | 23.3 | 69.2 | 13.6 |
| Effectiveness varies | 21.8 | 26.7 | 15.4 | 22.7 |
| Some but not much influence | 19.5 | 20.0 | 7.7 | 18.2 |
| None or very little influence | 32.2 | 23.3 | 0 | 45.5 |
| No influence but valuable for other reasons | 3.4 | 6.7 | 7.7 | 0 |
| *N* | 87 | 30 | 13 | 22 |

significant because Keeler apparently agrees that corporatism requires the presence of institutionalized group-state contacts. Institutionalization, it seems to me, must at a minimum mean that the relationship would survive changes in government or group leadership. I contend that the easy and near-complete dissolution of the corporatist features of the relationship between the agriculture community and the new Socialist government proves the French government's mastery of such contacts. When it chooses to work with the farmers or other groups because it shares their viewpoint or wants their electoral support, it involves those groups in the policymaking process; when it does not so choose, it can and does exclude them. Thus corporatist bodies are not the signs of an entirely new system of interest group-government interaction. These and other presumably corporatist forms of interest group-government interaction are simply some of a panoply of avenues of influence that French groups may resort to within what remains a complex pattern of interest groups politics.

FRANK L. WILSON

*Purdue University*

## Reply to Calvert and Wilson (Vol. 78, June 1984, pp. 496-497)

We are responding to Calvert and Wilson's (1984) critique of our article (van de Kragt, Orbell, & Dawes, 1983) which first came to our attention when it appeared in the *Review.*

Calvert and Wilson take issue, first, with our use of the term "public good" to describe the bonus we offered our subjects. We don't wish a terminological quibble, but our bonus was characterized by both *joint supply* (one person getting $10 did not prevent another from getting it) and by *impossibility of exclusion.* Of course, it was a "step level" good with some specified number of contributions being needed before provision, not an *n*-prisoners' dilemma such as the one offered in Calvert and Wilson's tangential table. But the logic of the minimal contributing set we offered depended on this step level characteristic. We attempted to be quite explicit that our model was not intended as a solution to linear dilemmas. If Calvert and Wilson wish to define a public good by the existence of a dominant strategy to defect, then that is their choice. But, we believe, their preferences hardly detract from the interest attached to the problem of providing a step level good that is jointly supplied and nonexclusionary.

Second, they argue that the minimal contributing set is not a "solution" in the sense "of being sufficient, merely by its existence, to drive participants to provide public goods optimally." Of course not. As Calvert and Wilson correctly saw, there are two equilibria in this game, one the Pareto-efficient "all contribute" and another the deficient one of "nobody contribute." We were at some pains to point out that a belief among set members that other set members would not contribute (for *whatever* reason) would make withholding contribution to their personal benefit. Contributing is not a dominant strategy; there are two equilibria; hence, neither is a necessary "solution." (In contrast to the Pareto-optimal equilibrium, the inferior one is the maximin solution—agreement or no agreement.) But the empirical result is that our subjects choose the Pareto-optimal one.

Calvert and Wilson also raise an "important epistemological issue." They point out that much work has been undertaken by economists and political scientists using the utility-maximizing model of participants and, it seems, they dislike our intrusion of "extra features" into explanations of behavior. Our model, *that members of an MCS will contribute because their contributions will be necessary (i.e., critical) for their own enjoyment of the public good,* is surely within the utility-maximizing tradition. The point is that free riding is impossible. What is not impossible is wasting one's contribution (if for *whatever* reason someone else does not contribute). The only "extra feature" beyond the standard utility maximization principle introduced by us is the hypothesis that this possibility of loss has no effect on behavior. That was our interpretation of the finding that the minimal contributing set mechanism (which might better be labelled the *designated minimal set* mechanism) works. That's standard utility analysis. When outcomes that have nonzero probability have no effect on behavior (and surely the probability that someone doesn't contribute is nonzero), their utility is considered trivial. Our finding demonstrated that within our context the possibility of free riding influences behavior, but the possibility of loss doesn't. The findings of these studies alone may be interpreted by assigning trivial utility to the possibility of loss.

In claiming that the MCS is not sufficient "merely by its existence" to ensure optimal provision, Calvert and Wilson make two elementary misreadings of our article. First, they argue that "the structure of the experiment . . . facilitates the *enforcement* (their italics) of such an agreement." Most decidedly it did not. Subjects made their decisions in strict anonymity, knowing they were anonymous; they were paid individually in a separate room from the room in which the experiment was carried on; prior subjects were well clear of the general area before subsequent ones were released from the experiment room and went to the payroom; finally, subjects knew in advance that prior subjects would be clear of the area. This was all made clear in our article.

Second, Calvert and Wilson argue, communication is crucial to optimal provision. Because communicating groups were never compared to noncommunicating ones, that inference cannot be drawn from the article. It turns out that Calvert and Wilson made a good guess. We have run 11 groups (nine players each) in which no communication was permitted and in which a minimal contributing set of five players was designated via a random drawing. The mean incidence of contributing was 83.6%, which is not 100% but compares favorably to the 48.8% when no contributing set was designated (van de Kragt et al., in press). Evidently eliminating the opportunity to free ride does contribute substantially toward group success even when the contributing set is not designated via discussion, but group success is short of the 100% observed in the experiments when it was specified via discussion. What role is discussion playing, therefore?

A further condition offered individual members of the MCS the bonus (regardless of what they personally did), if the other four contributed, with the result that *not contributing became a dominant strategy*. With the MCS again specified by a lottery, we observed a mean of 40.0% contributing—which does not reach the 96% contributing when discussion was permitted (five groups in each case)—but which compares favorably to the 28% when no MCS was specified (again five groups). Depending on one's perspective (is the glass half empty or half full?), the first finding might be taken as supporting predictions from the assumption of self-interest or not supporting them, but the fact that people contribute at all when their payoffs are independent of their own contribution suggests strongly that something beyond self-interest is involved, something greatly enhanced when discussion is present. Perhaps *it* is role designation, or perhaps increased concern for group welfare or generalized normative concerns. We are presently testing hypotheses phrased in such terms. But our point here is that such "extra features" do seem necessary to explain our later empirical findings and that theorists should not shrink from considering them.

We interpret our findings so far as strong evidence that decision rules harnessing self-interest to group welfare improves things greatly. But we also interpret them as saying that "extra features" are necessary for a full explanation of what happens. We know, for example, that an enforcement procedure is *not* a necessary "extra feature," contrary to what Calvert and Wilson assert.

Any disinclination to think about "extra features" seems to us more evidence of a religious disposition than of a scientific one.

ALPHONS J. C. van de KRAGT
JOHN ORBELL
ROBYN M. DAWES

*University of Oregon*

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## Comment on Carson and Oppenheimer (Vol. 78, March 1984, pp. 163-178)

I doubt that many of us who use the ADA or similar voting index to measure the personal ideology of congressmen are entirely happy with those measures, but the discipline is clearly better off if researchers continue using that type of measure rather than the one proposed by Carson and Oppenheimer in the March *Review.*

Carson and Oppenheimer point to two types of problems in using the voting-based indices of ideology: 1. overestimating the impact of ideology on voting—because voting-based indices reflect not only personal ideology, but also other variables, such as party, and 2. confusion of public and personal ideology—because the representative, in order to get reelected, may hide his true feelings.

Neither of these "problems" justifies a shift to the suggested measure. The first, overestimation, should never be a problem using voting-based indices. The second, confusion of public and personal ideolody, cannot be resolved by using the Carson and Oppenheimer measure, and a shift to that measure will create linguistic confusion in the discipline.

Overestimation of the impact of ideology is never a problem if the researcher uses control techniques that allow other variables to account for as much variance as possible before correlating the voting index with the voting behavior.

For example, if one is worried that the ADA index reflects party identification, one can run separate correlations for members of each party between ADA and the dependent variable, or one can run a partial correlation between ADA and the dependent variable controlling for party (and for any other variables of concern). Neither of those techniques could "over-count" ideology. In fact, the influence of personal ideology is *underestimated* by those techniques because personal ideology may have been one of the determinants of party identification. By looking separately at each party, or by letting party account for as much of the variance as possible before looking at the relationship between ideology and the vote, one removes some of the variance in voting that could be attributed to personal ideology.

Confusion between public and personal ideology is a problem, but one that cannot be resolved short of hypnotic investigation of each representative. Certainly we cannot resolve the problem by defining personal ideology to be uncorrelated with public behavior and memberships (as Carson and Oppenheimer do). Such a definition of personal ideology can only lead to linguistic validity problems. As used by others in the discipline (and by the public in general), the term "personal ideology" does not refer to a measure uncorrelated with party. In fact, many may assume that personal ideology is a prime factor in selecting a party. A measure, such as the one proposed, which classifies Cochran as the most liberal U.S. senator—because he is more liberal than other Republicans from Mississippi—will simply spread linguistic confusion.

The authors suggest this defect in their article—twice on p. 173 they note that we are not used to thinking of ideology in this way—and, in their conclusion, they say "we now identify those who go farther to the left of their constituents as the most liberal."

What Carson and Oppenheimer call personal ideology, others call ideological discrepancy. In their recent paper at the Midwest Political Science Association meetings, Covington, Fleisher, and Bond define ideological discrepancy as "the degree to which [representative's] voting records are too liberal or too conservative relative to the type of district they represent." In that paper, ideological discrepancy is measured using residuals from a regression relating district characteristics, party, and region to ADA.

There is just one theoretical model in which personal ideology and ideological discrepancy would be one and the same. That model assumes that representatives almost always vote the wishes of their constituents in an attempt to get reelected, and that any (usually minor) deviation from those wishes must be the result of personal ideology. There is no empirical support for such a model. Most current studies show that constituency has little impact on representatives' voting. Therefore, personal ideology must be distinguished from ideological discrepancy.

If any more proof is necessary that constituency has limited effect on representatives' voting, it is found in Table 1 of Carson and Oppenheimer. There they use, in their terms, a "kitchen sink" full of constituency indicators (32 in all) to predict voting behavior, and still get an $R^2$ of only .54 to .57. Thirty-two columns of random numbers would have done almost as well (I got .44 for 1980). There is no empirical justification for assuming the constituency-influence model, and therefore no justification for treating personal ideology and ideological discrepancy as one and the same.

Ideological discrepancy is a variable of great interest to political science—but it should not be confused with personal ideology. There is no benefit in encouraging researchers to substitute a measure of the former for a measure of the latter. It is confusing to call Cochran a liberal; it is not confusing to say that he is more liberal than his state and party would suggest.

ROBERT A. BERNSTEIN

*Texas A & M University*

## Comment on Carson and Oppenheimer (Vol. 78, March 1984, pp. 163-178)

I was intrigued by Carson and Oppenheimer's recent effort at estimating the personal ideology of political representatives. I was especially interested in the table showing the ideological scores of U.S. senators for 1979-1980. As they suggest, the table contains quite a few peculiar rankings, especially for a number of Democratic senators, usually thought of as liberals, who appear among the more conservative members of that body.

I have no basic quarrel with their approach; in fact, it makes good sense to control for the regional and constituency characteristics of elected officials to establish personal liberalism scores. I see no problem either in throwing almost everything imaginable into the regression equation to predict ADA scores. As they indicate, their purpose is to minimize the sum of squares of the residuals to produce scores that take account of every conceivable influence that might contribute to a senator's personal ideology. I do have a quarrel, however, with the inclusion of party in the equation.

As the authors make clear, they see party as a potentially major influence pushing a legislator either to the left or to the right. One of their examples mentions Weicker, the Connecticut Republican, whose party if not state push him to the right, but whose ADA scores of 72 and 68 reveal him as a strong liberal at odds with constituency preferences. Without closer examination, this makes sense. But let's consider another example or two to see what their technique produces.

Take the senators from Oklahoma—Boren and Bellmon. Somewhat surprisingly they have *identical* ADA scores for both 1979 and 1980, the years shown in the table. The two obviously share the same regional and constituency characteristics. Yet one (Boren) is considered a conservative (his score of −.40 ranks him as the 21st most conservative member), whereas Bellmon with a score of .26 appears to be a liberal (counting from the other direction he shows up as the 23rd most liberal senator). This gap of 53 ranks, based on identical ADA scores, apparently arises solely from the difference in party among the two. Or, consider the Texas example. Republican Tower (with ADA scores of 6 and 11) appears more liberal than his Democratic colleague Bentsen, who has ADA scores of 39 and 26. Again, the difference is obviously party. Would those knowledgeable of Texas politics really agree that Tower is more liberal than Bentsen? I don't think so.

This is not to argue that partisan differences are unimportant. To the contrary. I think Carson and Oppenheimer's inclusion of party in their prediction equation does not do justice to partisanship. As they admit, their technique "puts strong liberal labels on Republicans who are liberal." In fact, of the 41 Republican senators, 24 are found in the "liberal" column (59%). Is this as it should be? If Republican office holders across the country, or in the Senate, are generally more conservative than Democrats, which is what the regression equation indicates, why do we find a disproportionate number of Republican senators labelled as liberals? I think it would have been preferable to omit party from the equation and then perhaps test separately whether or not Senate Republicans are more conservative than Senate Democrats. This could be simply done, of course, using a dummy variable for party in a correlation with ideology or as a regression predictor of ideology. Then one could make an independent judgment about the effects of party. If this were done, I suspect that most Republicans in the Senate would be found among the ranks of the conservatives, where they belong. And such anomalies as Tower's more liberal score than Bentsen's would be avoided.

Granted that a residualized measure of a representative's personal ideology will likely yield some anomalous results, and no doubt partisan influences affect a senator's voting record, but to include a measure of party in an equation to predict ADA scores is guaranteed to generate ideological scores that obscure the effect of party and produce scores that are less useful than might otherwise be the case.

David R. Morgan

*University of Oklahoma*

## Comment on Carson and Oppenheimer (Vol. 78, March 1984, pp. 163-178)

The recent article by Richard Carson and Joe Oppenheimer, "A Method of Estimating the Personal Ideology of Political Representatives," (1984) attempts to create a more statistically useful measure of ideology than the more commonly used ADA ratings. But although what they do is statistically rigorous and justifiable, what they are left with, I fear, is not a measure of personal ideology, at least not if we consider ideology to be, as they do, "the usual convention of ideology along the liberal-conservative dimension" (p. 164, note 5).

The problem, it seems to me, is that although what they do makes statistical sense, from the point of view of measuring ideology it is not substantively reasonable. What they want to do is to create a measure that is free from the problem of multicollinearity with other factors such as party and constituency, and to do this they use a technique that allows them to "remove" the effects of these other factors and leaves us with a residualized measure of ideology. The difficulty with this technique is that the problem they are trying to resolve is not simply one of statistical collinearity, it is one of substantive collinearity as well.

Party and ideology are related. Demographic or economic (i.e., constituency) factors and ideology are related. There is no getting around that. Democrats are more liberal, on average, than Republicans; that is one of the reasons they become Democrats. To attempt to isolate completely which is the "liberal" influence and which is the "Democratic" influence is impossible, and to construct a measure of "liberalism" that is entirely independent of party is not possible. The residualized measure that they construct may be measuring something, but it is not liberalism. Any valid measure of ideology in contemporary America will be collinear with party and with certain demographic features of society. This presents statistical headaches, as we get unreliable estimates of coefficients in regressions using these measures. But I for one am afraid there is nothing that can be done about that.

One look at their table of "ideological scores" of U.S. senators (p. 174) shows the difficulties of using their measure. They note a number of "seemingly anomalous results" (p. 173), but I would contend that any instrument that has Strom Thurmond and John Stennis as two of the ten most "liberal" senators and Abe Ribicoff and Paul Tsongas as two of the ten most "conservative" senators is simply not measuring what it is supposed to be measuring. Liberal Democrats do not become "conservative" simply because they are Democrats from "liberal" northeastern states, and conservatives do not become "liberal" simply because they are southern Republicans.

Furthermore, a closer look at the authors' results and applications shows that they do not really solve the problem of separating the effects of party, constituency, and ideology. What they do simply shifts the problem from one of unreliable estimates to one of uninterpretable coefficients. For example, in discussing their final table the authors explain how using the ADA measure of ideology "decreases" the effect of party on labor issue votes (p. 177). The implication is that by looking at their measure we can get a more accurate measure of the impact of party. However, this is not true. As they make clear, the coefficient for "party" when using their measure of "ideology" is actually a coefficient for the effect of party *plus the aspects of ideology which are collinear with party.* Looking at their figures cannot tell us how much of that coefficient results from party, and how much of it is that portion of ideology which is related to party (and not, therefore, part of their measure of "ideology"). We are, then, left with the same problem as before. We cannot entirely separate the effect of the dependent variables because they are related to each other.

The authors finish by noting the "practical" significance of their measure. "It would permit . . . interest groups to assess which incumbents are 'friendly' and ought to be supported" (p. 177). By their standards, then, liberals should give money to Jesse Helms (he is more liberal than Gary Hart by their measure), and conservatives should help Bill Bradley (the most conservative man in the Senate) (p. 174). I for one would rather look at ADA ratings.

The intent of the authors is admirable. It would be nice to have an instrument to measure ideology which was not collinear with party and all of the other demographic characteristics they control for. It would make our job a lot easier as we could easily discover the independent effects of each of these factors. Unfortunately, social science is often, necessarily, a bit messy. We do have variables that are in part inseparable, which often leaves us with somewhat unreliable coefficient estimates. I am afraid that this is one of those circumstances. No amount of statistical manipulation can make that problem go away.

ARTHUR SANDERS

*Hamilton College*

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## A Reply to Our Critics

Our critics raise questions about the following aspects of "A Method of Estimating the Personal Ideology of Political Representatives": 1) our use of the term "personal ideology" and, indeed, the concept itself; 2) technical aspects of our estimation of personal ideology, and 3) anomalies in our

**Table 1. Original and Revised IDCC Scores with Actual and Predicted ADA Scores**

| | Decile of IDCC (1 = most conservative) | | ADA Ratings Actual (mean 1973-1980) | | Predicted ADA Rating (mean 1973-1980) | |
|---|---|---|---|---|---|---|
| | Old | New | Even Years | Odd Years | Even Years | Odd Years |
| Bradley (D-N.J.) | 1 | 1 | 72 | 68 | 88 | 86 |
| Cochran (R-Miss.) | 10 | 10 | 22 | 5 | 7 | 2 |
| Hart (D-Col.) | 6 | 9 | 67 | 81 | 73 | 78 |
| Helms (R-N.C.) | 7 | 5 | 7 | 1 | 6 | 3 |
| Ribicoff (D-Conn.) | 1 | 5 | 71 | 78 | 76 | 84 |
| Stennis (D-Miss.) | 10 | 3 | 6 | 5 | 36 | 33 |
| Thurmond (R-S.C.) | 10 | 5 | 5 | 2 | 11 | 4 |
| Tsongas (D-Mass.) | 1 | 2 | 89 | 74 | 93 | 91 |

rankings of the 1980 personal ideology of U.S. senators. We will consider each in turn.

As noted repeatedly in our article, "personal ideology" refers to the legislator's predilection to vote for or against particular bills statistically independently of the political constraints imposed by constituency and party. We argued that the more commonly used ADA measure (a summary voting statistic) is unable to measure the role of a legislator's ideology independently from the role of constituency and party. When the other components of the ADA measure are factored out by our residualization technique, however, it is possible to estimate the uniquely personal factor. The resulting measure demonstrates no more or no less than which legislators vote more or less liberally than one would expect given their party and constituency.

Because, as our critics point out, personal ideology has a lay meaning, it is difficult to keep the technical and common usage separate. We agree and now suggest the use of a more cumbersome but hopefully less confusing label for the concept: "ideological deviation from constituent/party characteristics" with the acronym IDCC. This formulation underlines that our measure is of ideology relative to party and constituency.

When Bernstein relates ideology to a congressman's choice of a party, he adopts a different definition of ideology from the one measured by IDCC. Given our focus on predicting congressional behavior, the role of ideology in determining party choice is not central to our concerns. He is correct that our measure does not allow us to deal with this issue.

Morgan raises the issue of whether or not ideology should be relativized to party; that is, should party be included in the equations used to estimate IDCC? This depends on the researcher's objectives. We see the usefulness of Morgan's point in some estimation situations and, even more clearly, in developing an index of how inherently liberal or conservative particular districts are without regard to the particular incumbent's party. Usually, however, the researcher intends to use party as a predictor of votes along with ideology. In these cases, party should be included in the equation used to estimate IDCC so that IDCC does not contain the influence of that variable. More generally, any predictor variable which the researcher plans to use in this manner should be in the IDCC equation.

Bernstein's statement that: "Most current studies show that constituency has little impact on representatives' voting" is too strong and the issue is not yet resolved in the literature. Certainly, a dominant theme in much of the literature (and most of the *economic* literature) on voting behavior is that members of Congress simply vote the interest of their constituents in order to get reelected and exhibit no independent ideology of their own (c.f. Peltzman, 1984, and the references in our article). We invite the reader to examine the recent article by Kalt and Zupan (1984) which develops a theory very similar to ours, which they apply to congressional voting on strip-mining legislation.[1]

The technical issues are raised mainly by Bernstein. He suggests that there are better techniques (especially partial correlations) for handling the multicollinearity problem and for controlling for the unwanted influence of other variables. Although there are multicollinearity problems with most of the voting model estimations we have seen, the major purpose of the IDCC method is to recover an inherently unobservable variable which then can be used in estimating voting models (c.f. pp. 166-169). Partial correlation coefficients are not a solution to the simultaneity or the latent variable problems. They are difficult to interpret in any but simple situations, and their use is inappropriate in structural models with even a moderate number of variables.

Bernstein's second technical point is that our estimates of two equations in our Table 1 are little better than he was able to obtain by regressing 1980 ADA scores against 32 random variables. Bernstein failed to note the number of observations in Table 1. He reports an $R^2$ of .44, which he compares to our reported $R^2$s in Table 1 of .57 and .54. However, our regression had 483 observations, owing to our method of stacking the different senate years. Because what matters is the ratio of estimated parameters to the number of observations, we obtained no $R^2$ greater than .1 when we used Bernstein's random number approach with 483 senators. Moreover, as was unfortunately unclear in the text of our article, we always used adjusted $R^2(\bar{R}^2)$ in order to correct for the ratio of estimated parameters to observations.[2]

Finally, what of the anomalies? In our article we suggest the anomalies in the table result from the particular specification of the IDCC estimation equations we use to illustrate the method and are not a product of the method itself. We also noted several ways in which that specification could be improved. Because these anomalies trouble our critics, it may be helpful to explore the results of new estimations which incorporate several of the suggestions made in our article.

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[1] We note here that Kalt and Zupan (1984) independently saw a similar need to divide ideology into two components, one due to constituents' interests and one due to deviations from constituents' interests. They also independently developed a method of estimating personal ideology which can be shown to be a special case of the more general method proposed in our article.

[2] The $\bar{R}^2$ from Bernstein's random example is only .18—less than half of the $R^2$ he reports—whereas our $\bar{R}^2$s are not much lower than our $R^2$s due to the large number of observations. We also note that the F-statistic corresponding to Bernstein's $R^2$ is just barely significant at the .05 level, whereas the $\bar{R}^2$ from our equations is significant at better than .0001 level.

In the original equations, all senators were constrained to respond to the same forces in the same way. Further, their IDCC values were based on a single session of Congress. This did not take into account the advantage of averaging across multiple sessions and ignored the likelihood that in the same district a Republican incumbent might be responding to different segments of the voters than would a Democrat incumbent because they have different coalitions. Our subsequent work uses average IDCC scores for 1973-1980 and allows for representatives to react differently to their constituency depending on their party, region, and size of their winning margin. For example, in a given district with $x$% blacks, our original equation forced every representative to react in the same fashion to this element of their constituency (and to all the others). Thus, in our new equation, the coefficient on the percent black for legislators outside the South is negative for Republicans, indicating a shift to the right as the percent black in a district increases. For the Democrats the coefficient is positive, indicating a shift to the left as the percent black increases.

Table 1 displays our original IDCC scores, our new IDCC scores, and the ADA scores for senators cited as anomalies by us in the article and by our present critics.[3] In each case the shift is in the direction of the score expected by those who viewed the original scores as anomalies. Ribicoff, for example, moves from being among those senators who are the most conservative, relative to their constituent-party (and electoral closeness) constraints, to a decile ranking that indicates that he is voting no more or no less liberal or conservatively than average. Similarly, Helms and Thurmond now have IDCC scores that show them to be reflecting their party and state constituencies instead of deviating from them in a strongly liberal direction. Hart moves to a substantially more liberal position, and Stennis moves from being very liberal to voting to the right of his constituents and party.

The scores for each of the three nonshifters—Bradley, Cochran, and Tsongas—have plausible explanations. Each was first elected to the Senate in 1978. Their IDCC scores therefore reflect their votes for only a single session. In each case, their ADA scores for that session were quite different from the scores predicted by our equation, thus earning them a berth among those with extreme IDCC scores. An examination of the ADA scores for their third and fourth years in office, however, shows that each shifted much closer to their predicted scores. Bradley's ADA scores for 1981 and 1982, for example, are 90 and 100; Cochran's were 10 for both years; Tsongas' were both 95.

This shows two things. First, for an overall understanding of a legislator's relationship to the pressures from his party and constituents his party-constituency voting constraints it is preferable, as we suggested in our article, to average IDCC scores over a number of years to avoid the influence of outliers. Second, the IDCC measure is a useful way to track the changing voting patterns of legislators over time. In the session immediately before his tough 1980 senatorial reelection campaign Hart, for example, shifted from his earlier liberal IDCC decile scores (e.g., 10 in 1975-1976 and 9 in 1977-1978) to 4, a much more conservative IDCC rating.[4]

The remaining cases are two pairs of senators from the same state; Bentsen (D) and Tower (R) from Texas and Boren (D) and Bellmon (R) from Oklahoma. In these cases what we have are not anomalies, but misunderstandings of the nature of the IDCC measure. Morgan asks the question, "Would those knowledgeable of Texas politics really agree that Tower is more liberal than Bentsen?", which he answers with a no and we agree. This, however, is not the question we sought to answer. Ours takes the form of: "Is Bentsen as liberal as his party and constituents would like or allow him to be?", a question that we would have to answer negatively if Bentsen's predecessors as Texas senators, Lyndon Johnson and Ralph Yarbrough, are considered. The Oklahoma case with Fred Harris is similar.

RICHARD T. CARSON

JOE OPPENHEIMER

*Resources for the Future*

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[3] Most of the senators' IDCC ratings did not experience large changes. However, owing to an error in our data source, the parties of Senators Leahy and Stafford from Vermont were reversed in our original article, a circumstance that affected their IDCC ratings.

[4] Thus the change in Hart's rating noted earlier is due to averaging, whereas those of Helms, Ribicoff, and Thurmond are primarily due to changes in equation specification. The change in Stennis's IDCC score is due to both factors.

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## Forthcoming in December

The following articles have been scheduled for publication in the December issue.

James E. Alt, "Political Parties, World Demand, and Unemployment: Domestic and International Sources of Economic Activity"

Jonathan Bendor, Serge Taylor, and Roland Van Gaalen, "Bureaucratic Expertise versus Legislative Authority: A Model of Deception and Monitoring in Budgeting"

Henry E. Brady and Paul M. Sniderman, "Attitude Attribution: A Group Basis for Political Reasoning"

Dagobert L. Brito and Michael D. Intriligator, "Conflict, War, and Redistribution"

John E. Chubb, "The Political Economy of Federalism"

Timothy E. Cook, "The Bear Market in Political Socialization and the Costs of Misunderstood Psychological Theories"

Arthur Denzau, William Riker, and Kenneth Shepsle, "Farquharson and Fenno: Sophisticated Voting and Home Style"

Terry Moe, "Control and Feedback in Economic Regulation: The Case of the NLRB"

Jonathan Riley, "On the Possibility of Liberal Democracy"

Philip Roeder, "Do New Soviet Leaders Really Make a Difference? Rethinking the 'Succession Connection' "

Joseph A. Schlesinger, "The New American Political Party"

Duncan Snidal, "Coordination versus Prisoners' Dilemma: Implications for International Cooperation and Regimes"

E. Spencer Wellhofer, "Two Nations: Class and Periphery in Late Victorian Britain, 1885-1910"

# BOOK REVIEWS

## American Government and Politics

**Nomination Politics: Party Activists and Presidential Choice.** By Alan I. Abramowitz and Walter J. Stone. (New York: Praeger Publishers, 1984. Pp. xiv + 158. $25.95.)

No distinction is more familiar in the party activism literature than that between "purist" and "pragmatist." For the most part, purists have been depicted as zealots ready to sacrifice electoral victory for ideological consistency. Several scholars have warned of the harm such types could do in the event they took over one or both of the major parties.

Are purists really so single-minded? Abramowitz and Stone contribute to the growing evidence of greater pragmatism among purists than originally thought. Their brief but important book offers a plausible model of how Democratic and Republican activists in caucus-convention states weigh perceived candidate chances in the general election against ideological agreement with candidates in deciding whom to support for the presidential nomination. Electability proved a much better predictor of activist choice than ideological congruence, but together these variables accounted for more than 85% of Democratic and Republican endorsements.

This finding is doubly interesting because big majorities in both parties subscribed to purist principles (as measured by stock but weakly correlated items). Purists evidently had taken over that part of the nominating process observed by Abramowitz and Stone, but most gave ground on ideology when necessary to embrace a more electable candidate. In sum, lip-service to purist ideals should not be taken too seriously. Most activists may want their view of good public policy to be enacted, but they recognize the importance of first winning office.

The data base for this study is enormous if nonrandom. Aided by numerous others, the authors collected 17,628 completed questionnaires from approximately 35,000 Democrats and Republicans attending 11 state conventions during the spring of 1980 for the purpose of selecting delegates to the presidential nominating conventions. Abramowitz and Stone ably use this statistical bounty to compare Democrats and Republicans, as well as candidate supporters within each party, on socioeconomic backgrounds, group affiliations, and issue stands.

Beyond quibbling with the general wording of certain questionnaire items, I have two criticisms of this study.

First, the authors use "utility" and "ideological satisfaction" interchangeably. Anticipating attacks from this quarter, they marshal strong correlations between points on their liberal-conservative scale and stands on specific issues. Even so, utility remains an empty vessel into which ideological proximity to candidates has been poured. Respondents never were directly asked what they expected to get out of supporting one candidate over another. An item or two of this type would have given utility at least a chance of conceptual independence, and the resulting data might well have reinforced the authors' approach.

Second, the analysis of activist decision making is basically static. By April, 1980, when Abramowitz and Stone first began collecting questionnaires, Reagan's nomination was all but certain and Carter's seemed almost as inevitable. Perhaps such widespread realizations figured in the scant support given Bush and Kennedy in the huge but late Abramowitz-Stone sample. What could an activist expect to derive from supporting an obviously doomed candidate, however close the ideological fit? Now it is unreasonable to fault the authors for not conducting earlier surveys, but a few recall questions to pinpoint the timing of support would have aided the analysis. At what point in the process did purists become more pragmatic about the candidates?

My own experience attests to how many good ideas come to mind *after* the data have been collected, and my criticisms are hardly fatal to this generally sound study. The Abramowitz-Stone model should be replicated in future research, in primary as well as caucus-convention states. It should be modified to take account of campaign dynamics such as the winnowing of candidates, and it should be tested in early contests where seven or eight candidates compete.

EMMETT H. BUELL, JR.

*Denison University*

**On What the Constitution Means.** By Sotirios A. Barber. (Baltimore: Johns Hopkins University Press, 1984. Pp. viii + 245. $17.50.)

Sotirios Barber has written an excellent book that seeks to present a general theory of "what the Constitution means." The book is really a statement of what is necessary for what Barber calls a "constitutional attitude" to exist in this nation. In order for there to be a constitutional attitude, the nation must do the following: engage in a continuous process of self-critical, aspirational, reaffirmation of the powers of constitutional government (chap. 4), constitutional rights (chap. 5), and constitutional institutions or rules such as separation of powers (chap. 6). This reaffirmation is not a homage to the law, judges, commentators on the law, nor to a contemporary rights *or* process theory of constitutional choice or one best moral position. Rather, constitutional rights, powers, and institutional norms are evaluated in the light of the goal of insuring that we, all of us, "give and exchange reasons for the ways in which we want to live with each other."

This quest requires that we do the following: honor rights as the most important constitutional obligation; reject self-interest as a basis of choice; oppose balancing as a means to decide rights and the nature of enumerated powers and institutions; continually seek a good society in congruence with the constitutional principles to which we aspire as a nation, knowing there is never an end to this process; and reject blind support for the Framers' policy wants and precedent. We are asked to view enumerated powers and the separation of powers doctrine, with checks and balances, not merely as means to stop abuse of power or rights violations but as praiseworthy in themselves, as an affirmative call to govern ourselves by claims to right reason.

The book is a devastating attack on the way modern constitutional theory and practice is done. Barber rejects: legal realist approaches to what the law is; self-interest as a way to talk about legal conflicts; both narrow interpretivism and wide-open noninterpretivism; consequentialism in the law; the Constitution as pure process values or proceduralism; balancing First Amendment rights with fears of a national emergency; acceptance by the Supreme Court and Congress of pretextual explanations of powers that don't fit the constitutional purpose of the enumerated powers (such as the commerce clause justification for public accommodations legislation); neutrality rather than the affirmative substantive responsibilities of Congress under the Civil War amendments; and judicial self-restraint and the failure of Congress and the president to take constitutional issues seriously.

Based on the above premises, Barber offers a superb analysis of why it is correct to allow abortion choice in the early months of pregnancy but not to consider such choices as a traditional right. He argues for abortion choice because those who oppose it have only come up with reasons that would stop the process of reaffirmation, the search, in its tracks while using religious-based moral ideas that run counter to rights premises in the First Amendment. Likewise, states rights ideas and leaving the allocation of choices on questions of race, gender, and ethnic equality to states are not favored, for they would result in the rejection of national constitutional standards in favor of state and local parochialism, thereby stopping the process of seeking the best society and constitutional values.

This book is a useful contribution to scholars and teachers of constitutional law, judicial politics, and the political philosophy of constitutionalism and the liberal state. Although it is an overstatement to call this work a general theory, it is excellent at creating a picture of how we can get away from the narrowness of the more legalistic means-ends conceptions of the Constitution and judicial choice and process and rights theories that dominate modern constitutional theory. It is also a forceful statement of the drawbacks of failing to take seriously such questions as whether institutions follow enumerated powers and institutional rules such as the separation of powers. It is a call to do so, to have a continuing national debate over the nature of our society and the type of constitutional principles and theory that is required so the debate will never end.

RONALD C. KAHN

*Oberlin College*

**Feeding Hungry People: Rulemaking in the Food Stamp Program.** By Jeffrey M. Berry. (New Brunswick, N.J.: Rutgers University Press, 1984. Pp. xiii + 182. $22.50.)

Hunger in the United States is a problem which grasps our attention periodically. A recent report by the Physicians Task Force on Hunger in America (*Hunger in America: The Growing Epidemic,* 1985) says hunger has reached "epidemic proportions," with up to 20 million Americans going hungry at some time each month. The physicians report that the problem is getting worse: Lines grow at soup kitchens and food banks, infant mortality rises, and there is widespread testimony about malnutrition. The physicians report that lack of food is not the cause of hunger. The experts attribute hunger to the lingering effects of the 1980's recession combined with

Reagan administration budget cutbacks in food and nutrition programs.

It is within this context that we read Jeffrey Berry's illuminating case study of rulemaking in the food stamp program. Berry begins with a cogent introduction to the regulatory context of social policy. Although we have had many studies of government's economic regulatory activities, Berry breaks new ground by examining social regulation. In particular, Berry looks at the development of issue networks (as replacement for the traditional "iron triangles" in public policymaking) and the role of administrative discretion. His case study is based on elite intervies with lobbyists and with major congressional and executive branch participants in food stamp policymaking, spanning 20 years from the early 1960s to the early 1980s.

Part 1 details the history of the food stamp program and the development of its regulations. The first chapter provides historical background. Food stamps were originally tried during the New Deal and the Second World War. Berry outlines the revival of the program in the 1960s, a period in which program administrators enjoyed great autonomy. Chapter 2 discusses the rise of hunger as a political issue in the 1960s, with the central role of the media. In the third chapter, Berry notes the increase in allotments and the decrease in purchase prices, largely through administrative rulemaking, during the Nixon administration. The fourth chapter discusses the changing fortunes of food stamps in the three most recent administrations, with major attention to the Reagan administration cutbacks and the role of interest groups in the rulemaking process.

The second part of the book places the case study in the larger context of legislative, administrative, and interest group behavior. Chapter 5 examines congressional involvement in administrative rulemaking. Berry's findings challenge many assumptions. Over the last 15 years, Congress has intervened more extensively in food stamp rule-making and has taken an active oversight role. The actions of many key congressional persons (Leonor Sullivan, Robert Kennedy, Jesse Helms) cannot be explained by traditional incentives. The final chapter treats the administrator's environment and administrative discretion. The chapter is especially good on detailing the rise of the hunger lobby and the impact of administrative guidelines.

In a brief epilogue, Berry notes the rise of hunger in the 1980s. He feels this is ironic, because government programs (especially food stamps) were, on balance, successful in reducing hunger in America in the 20 years before the Reagan administration budget cutbacks.

Overall, this is an excellent book. Berry teases our curiosity on some key points. For example, he presents a fine discussion of the rule-making process and bureaucratization (pp. 77-80). But, exactly when and how did things get so formalized so fast? *Feeding Hungry People* is, however, a lucid, readable, and informative book: It would be appropriate for courses in administrative law, public policy, and public administration if the publisher makes a paperback edition available.

ROBERT K. WHELAN

*University of New Orleans*

**A City in the Republic: Antebellum New York and the Origins of Machine Politics.** By Amy Bridges. (New York: Cambridge University Press, 1984. Pp. xi + 210. $29.95.)

The emergence of machine politics in New York and elsewhere, Amy Bridges suggests in this excellent book, was not solely the result of the ethnic conflict attendant to the massive migration into American cities in the antebellum period, for political machines antedated immigrant dominance of urban populations. Rather, this characteristic form of city political organization was the inadvertant result of the simultaneous presence of two powerful trends, one economic, the other political; one pervasive throughout the western world, the other particular in this period to the United States. The universal economic development was industrialization. The unique political one was democratization. As a consequence of democratization, and in contrast with the European experience, "the very first generation of the industrial working class had the vote in the United States. This meant that conflicts that were elsewhere social were here political" (p. 8).

This politicization of conflict connected with the fundamental alteration in the nature of work transformed the New York City polity, as Bridges demonstrates, from "mutualist" to "militant." In the earlier eighteenth century, city partisanship was not class-based; leadership in both major parties was by a business elite imbued with a communitarian ethic. Reflecting this ethic, the role of government included both economic regulation and relief for the poor pursuant to a notion of the common good. In contrast, by the mid-nineteenth century individualism and *laissez faire* were ascendent. City government became less a regulator and more a deliverer of basic services formerly made available through cooperative efforts. And the jobs created to provide these services allowed the separation of the urban from the national political system and provided the resource base for the political specialist—the boss—who came to mediate the relationship between the polity and the economy on the basis of a partisanship rooted in a new congruence between class and ethnicity. Completing the "characteristic antagonism" with which we are now so familiar, reform then emerged in reaction to the excesses of this system.

In elucidating her major themes, Bridges provides a striking discussion of nativism as less a xenophopic phenomenon than a transitional one reflective of the political resistance among artisans to economic change and the consequences of that change for their social and political roles. Also interesting and encouraging for those who both believe in the desirability of political competition and who seek a renewed role for parties in contemporary politics is the struggle described here by both Whigs and Democrats to sustain broad-based coalitions across class lines in a highly competitive urban political environment.

This volume is less a work about New York politics than about machine politics, with New York as an illustrative example. Yet those interested in contemporary New York politics will find here such familiar themes as efforts by upstate, Albany-based Republicans to bypass city government and gain control of service and patronage and fears expressed by reformers that the city's solid mechanics (read "business") would be "driven away . . . by unjust taxation, causing Westchester and other places to thrive" (p. 138).

This is a brief yet complex and closely argued volume. The research is meticulous. Bridges is comfortable with both statistical and documentary materials and imaginatively uses and combines evidence from various sources—the census, church records, and voting roles. Bridges is obviously comfortable with quantitative techniques yet judicious in her reliance upon them. And she takes care to regularly connect her findings to the secondary literature. This volume is a significant contribution as both a work of scholarship and as an exploration of the roots of modern American urban politics.

GERALD BENJAMIN

*Rockefeller Institute of Government,*
*State University of New York*

**Union Power and American Democracy: The UAW and the Democratic Party, 1935-1972.** By Dudley W. Buffa. (Ann Arbor: University of Michigan Press, 1984. Pp. vii + 268. $27.00.)

This book is about more prosaic matters than its title would indicate. Buffa describes how the UAW's relationship to the Michigan Democratic party has changed. Previously, the union respected a division of labor between the two wings of the labor movement and followed a policy of noninterference in party affairs. Now the UAW dominates the internal affairs of the state party. Buffa also traces the fortunes of the Michigan Democratic party from 1935 to 1972. Much of this material is trivial except to aficionados of Michigan politics, but in an unassuming way, Buffa's party history presents a microscopic view of the strains that tore apart the New Deal coalition. Unfortunately, this is done so unobtrusively that this reading of the book threatens to give it more credit than it asks for.

Buffa argues that in 1947 the passage of antilabor legislation at the state and federal levels piqued the interest of the UAW in the Michigan Democratic party. The UAW, however, was content to let the Michigan CIO take charge of rebuilding the party and making it a vehicle for the support of New Deal legislation. The state labor organization would represent the interests of labor to the state party. But in 1968 the UAW left the AFL-CIO and thus needed to forge its own links to the state party. In addition, the late sixties found the state party adrift and wracked with conflict. As blacks demanded more leadership positions, and policy conflicts broke out between the liberal and white working-class wings of the Michigan Democratic party, the UAW asserted itself. In 1972 the UAW dominated the state party convention, had its slate of candidates nominated for public office, and in 1973 installed a state party chairman that would do its bidding.

Buffa's view of these events is top-heavy. His argument is based on policy and personal conflicts among the state party leadership and the position the UAW leadership took in these debates. Buffa's institutional perspective on state party and labor politics may stem from his experience as assistant to the Michigan Speaker of the House and as a representative to the state party on the staff of Senator Philip Hart. Through these positions, Buffa was familiar with the labor and state party leadership, and he bases his argument on their account of events. But this aggrandizes the role of the leadership and diverts our attention from what is happening below the surface. For instance, there is little here on the problems the UAW encountered politically instructing its own membership. Buffa indicates that on broader issues than those of direct benefit to labor, "organized labor found it almost impossible to convince the members that the union leadership had any business entering the political arena" (p. 79). But he does not inquire as to how the leadership tried to overcome this condition, which limited their power and political maneuverability. When Buffa says white working-class voters in the suburbs supported George Wallace in the 1972 Democratic primary, it is as if none of these were

UAW members who supported a candidate the UAW rejected. Buffa does not examine the possibility that the closer the UAW leadership moved to the state party, the more the political gap between the leadership and membership increased. His focus on the machinations of the state party and labor leadership is too one-sided to capture this dynamic.

A less important error is his estimation of the power of a state AFL-CIO president. Buffa says Michigan AFL-CIO president August Scholle "wielded power over those below him in the union hierarchy" (p. 28) when, in fact, state AFL-CIO presidents have no authority over any labor people in their state except their immediate staff. Scholle was certainly among the more vigorous and colorful state AFL-CIO presidents, but whatever power he held was at the sufferance of affiliated local union leaders whose activity he could not direct.

Buffa succeeds in his coverage of personal and political struggles within the Michigan Democratic party. How much this enlightens us on the dynamics of union power and American democracy is still unclear.

ALAN DRAPER

*St. Lawrence University*

**Refugee Scholars in America: Their Impact and Their Experiences.** By Lewis A. Coser. (New Haven, Conn.: Yale University Press, 1984. Pp. xviii + 351. $25.00.)

This remarkable book represents an enormous investment in detailed learning and could only have been written by someone who, like Coser, is both a highly astute sociologist and himself a product of the historical processes that the book describes. Coser's central aim is to chronicle and evaluate the impact on American intellectual culture of central European refugee intellectuals who came here during the years between Hitler's rise to power and the end of World War II. It is very much a book of intellectual history: Coser rejects so-called objective measures of intellectual influence such as citation indexes and content analyses because, he believes, "a crucial dimension gets lost in an approach that neglects human agency, the interaction of human beings in the process of intellectual exchanges" (p. xi). Instead he opts for intellectual-cum-sociological biographies of a huge number of writers in seven major fields. In psychology and psychoanalysis, he deals, among others, with the Gestalt theorists Wolfgang Koehler, Kurt Koffka, and Max Wertheimer; with psychoanalysis, Charlotte and Karl Buehler, Bruno Bettleheim, Erich Fromm, and Karen Horney. The chapter on sociology and social thought deals with the Frankfurt School writers, the New School for Social Research, Paul Lazarsfeld, Alfred Schutz and phenomenological sociology, and Karl Wittfogel. The chapter on economics and economic history centers on Ludwig von Mises, Karl Polanyi, Jacob Marschak, Alexander Gershenkron, and various behavioral theorists. The chapter on political science and political theory has sections on Hannah Arendt, Franz Neumann, Leo Strauss, Karl Deutsch, Eric Vogelin, and Hans Morgenthau. The literature chapter deals mainly with Hermann Broch, Thomas Mann, and Vladimir Nabokov, although there is a more general discussion of refugee writers and the problems they faced in New York and Hollywood. An especially wide-ranging chapter on the humanities deals, among other things, with Roman Jakobson, Erwin Panofsky, and other refugee art historians; such literary scholars as Erich Auerbach, Leo Spitzer, and René Wellek; historians Felix Gilbert, Hajo Holborn, Hans Rosenberg, and Paul Kristeller; and with Werner Jaeger and the impact of European refugees on American classical scholarship. The final chapter, on philosophy, is dominated by Rudolph Carnap and the Vienna Circle, Aron Gurwitsch, and Paul Tillich. This catalog (by no means complete) should indicate the breadth and texture of the book, for although Coser is not attempting definitive intellectual biographies of his subjects but rather is focusing on the particular issue of their European heritage and its American impact, he exhibits a staggering control of literature over a huge diversity of fields, and it would be unjust not to take judicious note of this impressive fact.

Like all methods of doing intellectual history, Coser's has its shortcomings. The price of his remarkable breadth is exceedingly uneven treatment of some writers, and often tantalizingly little about the effect of the intellectual's refugeehood on either his or her own intellectual evolution, or on his or her impact on American intellectual culture (e.g., the section on Leo Strauss). It is not, in short, a book from which experts in the various fields can expect to learn much new about refugee scholars in those fields, and indeed its superficiality in areas one knows well can be quite irksome. But it remains a mine of anecdotal information and a source of much enlightenment.

A more serious criticism is that there is astonishingly little in the way of synthetic assessment of the various writers and the commonality of their refugee experiences. The reader's expectations in this regard are aroused by Coser's principles of selection: He excludes French intellectuals who, by and large, returned to Europe after the war; he excludes the natural sciences and the arts; he excludes those, like Ernst Cassirer, who died shortly after coming here, as well as refugee scholars who received their major education in America; and he concentrates almost exclusively on European scholars who had successful careers in America. Leaving aside questions that could be raised about these principles of selection (it could be argued, for instance, that European cultural influences were transmitted at a host of less visible levels), one would think that Coser would venture some generalizations deriving from the features his group shared in common. Yet there is no concluding chapter, and the 13-page introduction tells us little unexpected.

We discover that refugee scholars frequently could not satisfy their high aspirations, at least initially; that they often found the relatively low status ascribed to the professoriat in America difficult to accept, as did they the not-infrequent fact of lower positions in the status hierarchy than was the case for indigenous scholars of lesser achievement; that anti-Semitism was a problem; that younger scholars tended to adapt better; and that sometimes whole fields of achievement were not even acknowledged. We find that they were "most influential in areas of study where they filled a perceived need not previously met, or in fields in which they encountered an already established tradition to which they felt affinity" (p. 9). There is some brief discussion of Franz Neumann's sociology of types of refugee intellectuals, but the only substantive claim is that people fell into one or another group "not due to happenstance," but rather due to the "roots in the variant positions of these men and women in the social networks in which they were enmeshed and the social positions they variously occupied in America" (p. 12). This reader would have liked to see a good deal more in the way of explicit filling out of this kind of claim in the body of the work and a concluding assessment of the significance of his refugees' collective experiences.

In short, although the claim on the dust-jacket that this is a "vaulted gallery, crowded with superb word-portraits and vignettes" is amply justified by the book's contents, readers hoping for more will be disappointed.

IAN SHAPIRO

*Yale University*

**Presidential Primaries and Nominations.** By William Crotty and John S. Jackson III. (Washington, D.C.: Congressional Quarterly Press, 1985. Pp. xii + 251. $8.95, paper.)

This overview of the process of selecting presidential nominees is largely a routine compilation of existing literature and available empirical findings. Its primary use would seem to be as an undergraduate text. Even in that role, however, its usefulness is limited by a lack of incisiveness in analysis or writing; it provides only a basic background on the presidential nomination process as the basis for class discussions.

The book is organized in three sections totaling nine chapters. Part 1 outlines the historical succession of nomination systems through the party reforms of the 1960s and 1970s to the mid-eighties. Part 2 deals with participants in primaries/caucuses, convention delegates, candidate strategy, campaign finance, and the conventions. Part 3 is a single, brief chapter on proposals for further reform.

The book covers a large number of relevant and appropriate topics. But they are seldom fleshed out sufficiently, their relevance is often unclear or underdeveloped, and sections seldom fit cleanly together, even within chapters. Handling of normative questions is particularly weak, with arguments from both sides often followed by a statement that readers will of course want to make up their own minds based on their own values.

One general problem with the book is the frequent failure to place the analysis in an appropriate political context. This is particularly troublesome in the authors' treatment of representation. They make the standard distinction between demographic and attitudinal representativeness, but the two are analyzed separately with almost no attention to the linkage between them. There is no real discussion of the interests or demands of the various social groups mentioned, and criteria for selecting groups for attention are often unclear. Crotty and Jackson do point out that "young people in general are not a coherent group with shared long-term interests" (p. 109), but they fail to realize that explicit attention to the question of who constitutes such a meaningful group and how such groups are related to each other is necessary to decide which groups to study in the first place. The discussion of quotas for minorities and women, for example, fails to make clear why *these* were the groups within the party whose representation became politically important. The disembodied tone of the discussion becomes clear in a much shorter section on the Republicans in which reform is approached in terms of increasing access of precisely the same groups as were discussed for the Democrats. Explicit concern with attitudinal representation deals largely with ideological self-placement of delegates, which the authors note shifts from year to year. However, failure to place this in the context of the internal politics of the party—for example, such as the nature of the Carter presidency and of the Kennedy challenge to it in 1980

—makes it a rather weak (if accurate) conclusion.

Chapter 6 is a particularly disappointing discussion of voting behavior and candidate strategy. The authors deal with individual vote choice in nine pages, referring very briefly to concepts such as candidate image, ideology, and issues without defining them clearly or analyzing them in any detail. The evidence is almost entirely anecdotal. The discussion of candidate strategy is a checklist of relevant factors, the role of which is highly underdeveloped. A conclusion is that "personality and ideology have importance only relative to other factors and the field of candidates" (p. 152). The chapter ends with a comment that "we have barely touched the complexity of presidential nomination campaigning" (p. 156). The chapter should have been the central one of the book where assorted themes could be brought together and focused; instead, it is the weakest in the book.

The book tends to be less than incisive in confronting normative questions. Chapter 4, for example, which deals with primaries and caucuses as alternate delegate selection procedures, includes as comments late in the discussion that "considerable controversy rages over the issue of whether caucuses do a better job of nomination as compared to the primaries" (p. 97). Furthermore, "there are no definitive answers to these controversies. Trade-offs must be made in accepting either position. There are some advantages to mass participation and there are some advantages to elite-dominant decision making in the nominating process. The American solution is often to compromise" (p. 100). One might have hoped for analysis to focus on precisely what the controversy consists of and on what, exactly, the trade-offs are.

DAVID G. LAWRENCE

*Fordham University*

**Problem Definition in Policy Analysis.** By David Dery. (Lawrence: University Press of Kansas, 1984. Pp. xiv + 145. $19.95.)

The recommendations that emerge from policy analysis hinge on the way the policy problem is framed in the first place. Yet the nature of policy problems is elusive. David Dery explores that mystery much as his teacher, Aaron Wildavsky, did in *Speaking Truth to Power* (Little, Brown, 1979).

His main point, like Wildavsky's, is to attack "rationalist" notions that problems are somehow "given" and analysis is confined to the selection of means to attain them. The choice of problems in fact hinges on politics, many definitions are possible, and each implicitly points towards certain remedies. Problems and solutions thus are chosen together, not separately; "problem definition is a framework within which certain interventions are considered—and indeed defined—as solutions" (p. 5). When policies fail, the sensible course is usually to recast goals, not reform programs. "Policy analysis consists, not of replacing ineffective behavior with efficient means to the same ends, but of learning from experience what we should prefer" (p. 20).

However, the choice of problem is not entirely subjective. According to Dery's "interventionist perspective," a true *policy* problem cannot be a dilemma with no apparent answer, like urban crime or congestion. The problem must have practicable solutions, and their overall benefits for society must exceed their costs. Admittedly, this will seldom mean net benefit in Pareto terms. Policy problems are usually "complex," involving conflicting values; a political decision to serve some and hurt others must be made. But in some overall sense, a policy problem must offer "opportunities for improvement."

In this connection, Dery endorses the Berkeley view that implementation failures are not scandals to be rooted out. They arise because the proponents of programs favor one set of values, their opponents another. The route to better implementation is political learning and problem redefinition, not tougher-minded administration.

The main impediment to learning is what Dery calls the "handicapping force of organizations" (p. 117). Problem definition typically occurs "within policy-making arenas" (p. 117) dominated by bureaucracy. Agencies defend established definitions because existing programs are linked to them, while redefinition might mean new programs controlled by others. The "enlightenment model" of analysis, accordingly to which analysts lead policymakers by an educative process, is usually naive. To have influence, experts must be prepared to engage in "persuasion and contestation," not just research (p. 118).

Dery writes clearly, and occasionally with wit. He succeeds in conveying the ineffable way policy problems already look toward solutions—the sense in which "the problem is itself a model" (p. 7). Some of his discussion is illustrated through reference to youth and other social programs he has studied in Israel where he teaches. Most of the book, however, is an extended commentary on the existing literature on problem definition. It is an exercise more in criticism than research. Thus, it is broken up with references to other authors and digressions about them. This makes it difficult for the uninitiated to follow. The organization is also repetitive, with successive chapters saying much the same thing in similar ways.

This book should be of interest to those involved in problem definition, a field it covers with authority. Readers distant from that subject, however, will probably wonder how much Dery has really added to Wildavsky's fundamental insight, that ends and means in policymaking are, in a sense, the same. An in-depth case study of problem definition and redefinition in one of the programs Dery mentions might well have taught us more.

LAWRENCE M. MEAD

*New York University*

**Political Women: Current Roles in State and Local Government.** Edited by Janet A. Flammang. (Beverly Hills, Calif.: Sage Publications, 1984. Pp. 320. $28.00, cloth; $14.00, paper.)

This collection is an important contribution focusing attention on women's political involvement in state and local politics as a complement to more readily available analyses of the national legislature. With a careful but persistent feminist tone throughout, the book's contributors sustain theses that women are transforming traditional political concepts, female officials have (or develop) distinctive politics, parties remain problematic for women, and there is political significance in women's initial inclusion in government (if even as tokens) and as a majority of voters at the polls.

The transformation of political concepts as a result of women's participation in state and local politics is a focus of Diane L. Fowlkes's perceptive analysis of how women's political activism is based on "extrinsic" accounts of connectedness outside the political actor, as well as traditional "intrinsic" definitions locating agency internally. Marianne Githens provides an interesting assessment of how differential background factors—such as socialization and situational constraints—affect the legislative performance of women in state politics, thereby introducing new sets of policy concerns. Martha A. Ackelsberg illuminates how the traditional split between the politics of work and the politics of community is challenged by women's issues, which necessarily require an integration of both spheres. Edmond Constantini and Julie Dairs Bell present provocative findings about male/female motivational differences in the context of state legislative politics and public policy formation.

The development of distinctive politics and policies in response to women's issues is illustrated by Anne Wurr's unusually fine case study about how women in Santa Clara County, California organized a Support Network, educated establishment police officials, and eventually instituted policies at the local level to ensure greater police effectiveness in protecting women from domestic violence. Denise Antolini argues that local female officials offer perspectives to the policymaking process directly derivative of a growing liberal and feminist orientation.

The role of political parties and the difficulties faced by women to mobilize this resource for their political ambitions is discussed by Flammang, who evaluates how women's groups perform party-like functions at the local, grass-root level, which in turn critically assist development of public policies in legislatures at the state level. How female officials develop successful political strategies outside traditional sources of party support is presented in Carol Mueller's excellent analysis of collective strategies employed by the now "critical mass" of women in state legislatures, thereby illuminated intricacies of developing and sustaining new political coalitions by and for women. Janet Clark et al. present fascinating data on the electoral prospects of women candidates in different electoral environments and in relation to measures of voter hostility and incumbency.

The significance of women breaking through political barriers is impressively documented by Beverly B. Cook in an examination of the state and local court system in relation to women's access to the judicial branch of government. David B. Hill considers the thesis that the relatively smaller number of women in state senates can nevertheless be disproportionately effective as cue givers to others in the policy formation process. Emily Stoper provides an excellent overview of state and local policies in relation to national politics on the subjects of abortion, child care, divorce, and welfare—or, the politics of motherhood—pointing to the unique problems and solutions necessarily of concern to women. Ellen Boneparth contributes an insightful overview of the relation and significance of state and local politics to the national level in an age claiming the value of "dual federalism."

As can be discerned from this brief synopsis, this volume covers a wide range of topics. One cautionary note is in order concerning the organization of such diverse contributions. Sections do not necessarily correspond to stated themes and sometimes are misleading: The section, "Female Officials in State and Local Governments," in fact, does not contain any chapters dealing with local government (only state). More important, although chapters exhibit individual strengths, systematic conceptionalization relating chapters to each other, and a formal assessment of distinctive features of state versus local levels of government in relation to women's issues and women's participation in elected elites, remain largely undeveloped. However, with these reservations aside, *Political Women* is recommended as a long-overdue set of readings filling an important literature gap.

EILEEN L. MCDONAGH

*Northeastern University*

**The Costs of Federalism: Essays in Honor of James W. Fesler.** Edited by Robert T. Golembiewski and Aaron Wildavsky. (New Brunswick, N.J.: Transaction Books, 1984. Pp. 330. $29.95.)

This *festschrift* for James W. Fesler provides a wide variety of perspectives on federalism, broadly defined. As with so many contemporary essay collections, there is no single theme or conceptual framework to unite these analyses. Although the book's title implies a focus on the "costs" of federalism, none of the essays addresses this issue explicitly. Some of the essays do discuss consequences which their authors view as costs, yet there is an equal interest in federalism's benefits, and some essays treat federalism as a dependent variable in considering the conditions that make it possible. Nor do the essays assume a single conceptualization of federalism. In his introductory essay, Wildavsky offers an umbrella definition of federalism as "the diverse organizational elements of modern pluralist democracy" (p. 4) which is necessary to encompass the variety of subjects discussed. The essays range from Nelson Polsby's analysis of the prospects for American pluralism, which does not (except in the essay's title) mention federal structure, to Fred Greenstein's essay on President Eisenhower's views on administrative delegation, to Carolyn J. Tuohy and Robert G. Evans' discussion of decentralized health planning in Ontario. Although there is no unifying theme in this volume, these original essays are of high quality and taken together or individually will be of interest to political scientists with divergent concerns.

The weaker of these essays are in a section titled "A Plurality of Conceptions" (pp. 21-69), which includes the Polsby essay mentioned above and contributions from Theodore Lowi and Wildavsky. Lowi argues that the historical absence of a socialist movement in the United States is largely a consequence of the federal structure, but his claim to the originality of this thesis is undermined by his own quotations from Madison's *Federalist #10.* Wildavsky's essay, "Federalism Means Inequality," is full of intriguing insight but suffers from a fuzzy conceptualization of equality defined largely through the familiar distinction between equality of "opportunity" and "result." He fails to explain why his conclusion that "federalism and equality of result cannot coexist" (p. 68) applies only to "result." Federalism's role as a support for segregation in the South suggests otherwise.

The best of the volume's essays offer empirical descriptions of centralization/decentralization issues in contemporary regimes. Two describe recent developments in American intergovernmental relations: First, Donald Kettl offers a model of the segmentation of intergovernmental politics between congressional preoccupation with distribution, issue network dominance of substantive ends, and third-party dominance, often in the form of nonprofit organizations, of service delivery. Second, David Caputo traces the impact on American cities of first the expansion then the reduction of federal aid to the cities. In an essay on campaign finance reforms, Herbert Alexander argues that recent efforts by both the Republican and Democratic parties to increase centralized fund raising through the party organization are not likely to alter significantly the highly decentralized character of campaign funding sources. A comparative perspective on these American developments can be found in Alfred Diamant's elegant description of the French Socialist government's program of administrative decentralization. He says these reforms, although grounded in incremental evolution in the postwar period, are dependent on "continued Socialist political control at all levels of government" (p. 162), an increasingly problematic situation.

Rounding out the book are three proposals for reform: One is Garry Brewer's proposal for "termination" as the missing ingredient in the recent industrial policy debate. Second, Golembiewski suggests a comprehensive shift from functional to areal organization in the federal bureaucracy. And, finally, James E. Swiss offers practical recommendations for improving administrative efficiency in the food stamp and AFDC programs.

In sum, the diversity of topics discussed in this book is its virtue, and perhaps a reflection of the man in whose honor it was written. In his concluding essay, Golembiewski emphasizes Fesler's ability as a teacher to "seek value from multiple orientations and perspectives" and to "encourage us to follow our methods of choice, while inspiring us with his standard of excellence" (p. 299). These abilities have clearly born fruit in the work of his students presented in this volume demonstrating that diversity, a principle benefit of federalism, has also been a benefit of Fesler's legacy to political science.

WILLIAM E. HUDSON

*Providence College*

**Knowledge and Discretion in Government Regulation.** By Ted Greenwood. (New York: Praeger Publishers, 1984. Pp. xii + 283. $34.95.)

Trained originally as a physicist, Greenwood has specialized in science and technology policy during his career in political science. He therefore brings an interesting perspective to government regulation concerning risk assessment of health or safety standards.

This book is based in part on interviews with "numerous" public and private officials focused on the EPA hazardous air pollutant program and the OSHA occupational health program. It is a revision of a 1981 report conducted under contract with the Office of Science and Technology Policy. The various chapter notes indicate a substantial review of the literature, relevant statutes or regulations, and major court cases. Therefore, the book is clearly an interesting and distinctive contribution to the science policy literature and helps extend that literature to issues of government regulation.

Its objective is, by "studying the interaction" between knowledge and discretion, to generate "a set of observations and an analytical framework useful for understanding and analyzing any public policy arena where science or engineering knowledge plays an important role" (p. vii). A central issue is that "the boundary between knowledge and discretion in regulation is fuzzy and their interaction is complex" (p. 221).

Knowledge means the state of scientific and engineering theory or information concerning a standard and what potential risks will be involved. "The central role of scientific and engineering knowledge is one of the most prominent features of environmental, health, and safety regulation and sets it apart from many other areas of social policy" (p. 273). More critically, this central role is greatly affected by "conflicting interpretations of deficient knowledge" (p. 2). Scientific knowledge is neither complete nor reliable. Nevertheless Greenwood concludes that federal agencies are generally competent in assembling and applying available scientific knowledge to final standards. "Final action by regulatory agencies is usually well grounded on scientific and engineering knowledge" (p. 273). The degree of success is affected by such factors as agency personnel, structure, and procedures—but these factors are controllable.

The crucial problem lies in agency discretion (granted statutorily by Congress) to set standards based on administrative judgment. The book is therefore directed at "the nature of administrative discretion, how it is exercised, and how and to what extent it is constrained by knowledge" (p. 3). Values, policy orientations, and political considerations have a major role to play, because discretion must necessarily supplement deficient knowledge. "The most important causes of controversy in the regulatory arena are the exercise of discretion and procedural inadequacies" (p. 273). There are at least four dimensions to discretion: the ability to interpret statutory language, the right to balance conflicting values, the freedom to determine priorities, and the need to answer scientific issues.

The book contains valuable information on the two programs studied, and Greenwood's insights into the policy process are useful. However I found the presentation is to be unbalanced. The information and insights seemed to be subordinated to the author's clearly expressed goal of creating a general analytical framework capable of application to other policy arenas dependent on scientific information. The general framework overwhelmed the two policy areas studied. As a result, I found much of the presentation stilted. There is too much exposition of highly abstract arguments. I do not mean to be critical of the basic research involved: Much of Max Weber's most seminal work in sociology could be described in exactly the same terms. But the book is basic rather than applied research, despite its empirical foundation and policy orientation. In this sense, the book strikes me as unbalanced.

DUANE WINDSOR

*Rice University*

**Presidents, Politics, and Policy.** By Erwin C. Hargrove and Michael Nelson. (Baltimore: Johns Hopkins University Press, 1984. Pp. xii + 288. $25.00, cloth; New York: Alfred A. Knopf, 1984. $11.95, paper.)

Scholars of the presidency have viewed presidential influence as a reflection of a wide and quite varied range of factors: his constitutional powers, abilities as a strategic bargainer, personality, organizational skills, and popularity with the public, to list only a few. But few attempts have been undertaken to integrate these disparate insights into a coherent account of the modern presidency. Hargrove and Nelson attempt this task in *Presidents, Politics, and Policy*; by and large they succeed.

Although Hargrove and Nelson do not present any new empirical research, they do offer a well-crafted critique and synthesis of relevant literature. Moreover, their perspective, analysis, and conclusions are thoughtful and illuminating. They begin by pointing out that most empirical accounts of the presidency are incorrect in describing its power as wholly weak or strong; from a normative perspective, the office cannot be assessed solely in a positive or negative light. Thus, the presidency should not be viewed either as "savior" (strong empirically, positive normatively), "satan" (strong empirically, negative normatively), "samson" (weak empirically, positive normatively), or "seraph" (weak empirically, negative normatively).

The basic thesis of the book is that presidential influence, properly understood, is multistranded and context bound. Rather than select one factor as determinative, Hargrove and Nelson argue that constitutional powers, political culture, and leadership skills must all be considered in analyzing presidential politics. Furthermore, they argue that a proper understanding of presidential influence should be historically grounded: Different historical periods call for different kinds of presidencies. A cycle of presidential influence exists, with particular types of influence appropriate to different periods in the cycle. Presidencies of "preparation" (Theodore Roosevelt, Kennedy, and Carter) anticipate new periods in American politics. They are followed by presidencies of "achievement" (Wilson, Franklin Roosevelt, Johnson, and Reagan), in which bursts of creative energy alter the bounds of public policy. Finally come presidencies of "consolidation" (Hoover, Eisenhower, and Nixon), in which reform is stabilized. Not all presidents meet the needs of the times. Presidencies of "stasis" (Ford), characterized by drift and confusion, can emerge. Incumbents also can fail to exercise influence in effective and proper ways: the "flawed" presidencies of Johnson and Nixon.

Successive chapters illustrate the impact of this cycle in situations where exercises of influence are crucial: the selection process, management of policy formulation, executive-congressional relationships, and management of policy implementation. With respect to relationships with Congress, for example, Hargrove and Nelson point out that presidencies of preparation need well-honed rhetorical skills, whereas presidencies of achievement require skills of bargaining and an ability to form political coalitions. Less satisfying is the authors' contention that tactical skills are largely irrelevant to presidencies of consolidation (p. 229). As the recent revisionist literature on Eisenhower indicates, tactical skills are not irrelevant as much as they need to be more covert, selective, and indirect. Virtually no attention is given to how politics varies within a particular historical period, and thus how presidential influence must be tailored to the differing array of political forces the president faces in different policy areas.

What forces "drive" this historical cycle through its different stages especially requires further analysis. Hargrove and Nelson do contend that elections are the "engine," ideas the "fuel," and leadership the "carburetor." But this is rather sketchy mechanics; much more is needed by way of structural or causal analysis. Furthermore, the placement of particular presidencies in particular segments of the cycle could benefit from more critical discussion. Carter, for example, is classified as a president of preparation, whereas Ford is viewed as a failure. Other scholars such as James David Barber, Bert Rockman, and Stephen Skowronek have toyed with the notion of historical cycles, but with markedly different results. Skowronek, for example, proposes a cycle of regime construction, regime management, and regime enervation, with the Carter presidency now viewed as an enervated regime and Kennedy labeled as a regime manager. The authors and their editors also must be faulted for that most grievous of sins committable by scholars of the modern presidency: adding a period after the middle initial in Harry S Truman.

But that said, the work is indeed rich in original insights and observations about the presidency as well as in basic fact and detail. As such, it makes a welcome contribution to contemporary scholarship, as well as a thought-provoking discussion that is readily accessible to students.

JOHN P. BURKE

*The University of Vermont*

**The Government/Press Connection: Press Officers and Their Offices.** By Stephen Hess. (Washington, D.C.: Brookings Institution, 1984. Pp. xv + 160. $22.95, cloth; $8.95, paper.)

After spending a year inside the press offices of five federal agencies in Washington (the White House, State Department, Pentagon, Department of Transportation, and Food and Drug Administration), Stephen Hess presents his experiential findings in this compact but revealing volume.

A major portion of Hess's work is devoted to a discussion of the characteristics and functions of press secretaries and press officers. He finds little real conflict in loyalties between the press secretaries, who are political appointees, and the subunit press officers, who are career personnel. Instead, he reports that most of the infighting he witnessed involved political appointees within an agency and notes that career officers had a real interest in bureaucratic politics but rarely in partisan battles.

Not surprising is the notion that reporters believe that access to an agency head is what makes a press secretary a good one. To be in the inner circle ("the loop," in Washington parlance) would seem to be essential for effective and honest press secretary performance. Hess notes, however, that it is common practice in government for agency heads to keep their press spokesmen uninformed in order to protect them from having to lie.

One of the more interesting features of this volume is the contrast in media activity it draws between the White House and other executive agencies. The typical government agency initiates very few "events," whereas the White House is the center of media activity. Significantly, the White House press office is oriented toward television, while other agencies (State, Defense, etc.) are essentially "print beats" (p. 103). During the Reagan administration, this fact has been underscored heavily with the administration often advancing a particular "line of the day," which the president's publicity team pushes and which often becomes the focus of evening network news shows.

Because Hess's experience was centered from 1981 to 1982, his comments about the Reagan administration carry particular relevance. Hess takes an unconventional view of the administration's decision to bar the press from the island of Grenada during the U.S. invasion in the fall of 1983. He writes that the impact of the administration's policy "redirected the press to get its news from Radio Havana, amateur radio operators, rumor mills, and political opponents. . . ." and concludes that "the price paid by the nation for the administration's press policy . . . was less accurate news. The price paid by the administration was fewer favorable stories" (p. 60).

Another painful lesson learned by the Reagan administration as well as its predecessors is that leaks—and Hess distinguishes among six different varieties—not only can be quite damaging and irritating to the president but also tend to emanate from the administration's own political appointees, especially perhaps from the most ideologically oriented among them.

During the first Reagan term, the White House became so upset over "leaks" that it announced that any unauthorized disclosures would result in officials being investigated, and that the investigations would "include the use of all legal methods" (p. 90).

Despite the occasional furor over leaks, Hess makes two telling points. First, the government is quite adept at keeping secret what it really wishes; second, it might not always be in the president's interest to stop leaks because many are in his favor.

One additional aspect of Hess's work is worthy of mention. The charge is often made that government press offices manage, manipulate, and control the news. Hess argues, however, that such offices are neither skillful enough nor large enough to accomplish such difficult goals and that instead they spend much of their time and energy at such basic tasks as collecting material requested by reporters and attempting to learn what their entire agency is doing.

It should be noted that Hess's press office observations were confined to the early Reagan administration rather than spanning two or more administrations or even the entire duration of one presidency. A comparative analysis might have been helpful here, although Hess's findings (which, he indicates, were tested through interviews with reporters and civil servants from previous administrations) seem likely to stand. Although greater elaboration of some of his points and more frequent reference to other administrations would have been valuable, Hess has produced a useful and occasionally provocative contribution to the growing but still inadequate body of literature that focuses on the symbiotic relationship between government and the press.

ROBERT E. GILBERT

*Northeastern University*

**The Industrial Policy Debate.** Edited by Chalmers Johnson. (San Francisco: Institute for Contemporary Studies, 1984. Pp. x + 275. $21.95, cloth; $8.95, paper.)

The 10 essays edited by Chalmers Johnson in this book do not represent, as the title might suggest, a cross-section of differing views on the design and implementation of industrial policy in the United States. Rather, they cohere around the single perspective described by Johnson as "recognition that the government-business relationship in the United States has become a serious, costly drag on American economic competitiveness in the world" (p. 3). The results overall are the expression of considerable suspicion about government's ability to produce and sustain policies that have positive consequences for American economic competitiveness and a very narrow rationale for why any industrial policy should even be attempted at all. Consequently, I found it difficult to agree with Johnson's characterization of his colleagues' work as "a broadly based, non-partisan discussion" of a controversial and important topic.

A fairly broad range of particular issues in industrial policy is addressed, however, albeit with varying degrees of success. In addition to Johnson's introductory chapter, "The Idea of Industrial Policy," essays by Eugene Bardach on "Implementing Industrial Policy," Regis McKenna on "Sustaining the Innovation Process in America," and Paul Seabury on "Industrial Policy and National Defense" offer the reader the most stimulating and useful insights at both the conceptual and practical levels. I would especially cite Bardach's discussion of the political pressures likely to be faced by a hypothetical "Agency for Industrial Policy" and its subsidiary "Bank" for industrial loans as the book's most articulate challenge to advocates of an industrial policy managed by a pro-active, interventionist government.

On the other hand, Melvyn Krauss's "'Europeanizing' the U.S. Economy" and Bruce Bartlett's "Trade Policy and the Dangers of Protectionism" are the most problematic expressions of the book's partisan biases. Bartlett's argument, directed against those calling for government protection of disadvantaged or weak domestic industries, is that "the trade deficit is really a meaningless concept" (p. 167). The argument is neither well documented nor sensible in the context of what is practiced by America's foreign competitors—especially protectionist Japan.

Even more questionable is Krauss's loose interpretation of historical facts to try and prove that current "neoliberal" industrial policy—as proposed by Robert B. Reich, Lester Thurow, and Felix Rohatyn—is a new form of corporatism that Krauss labels "fashionable fascism" (p. 72). There is, he asserts, "an explicit institutional parallel linking the neoliberal version of industrial policy with the New Deal and the Mussolini era," primarily through Felix Rohatyn's proposal to establish a new Reconstruction Finance Corporation modeled on "the early Roosevelt version." According to Krauss, "the original RFC was inspired by Mussolini's Instituto per la Ricostruzione Industriale . . . established in January 1933 to combat the severe Italian recession" (p. 72). Krauss does not mention that the *original* RFC was created in 1932 by Herbert Hoover to combat the worsening American depression that began during his administration. What this might mean for the issue of who actually inspired what in the evolution of industrial policy is a question that Krauss does not pursue.

It is unfortunate that *The Industrial Policy Debate* has no strong statement of the neoliberal position or any other position on industrial policy that could take issue with Bartlett, Krauss, and others in the book who argue that the best industrial policy may be no industrial policy at all. National defense considerations and tax reform opportunities are the sole positive arguments made for attempting industrial policy, and this amounts to a timid debate indeed. Johnson, who avows his support for the development of an American industrial policy, is left to conclude from his colleagues' contributions that industrial policy is only "a viewpoint, one similar to that of the coach of a football team who has to work both on fundamentals in training his men and on producing a game plan for each individual contest" (p. 241). It is a weak characterization of a complex idea that reflects the limitations of this book more than the idea itself.

DAVID MENNINGER

*University of California, Los Angeles*

**Judicial Policies: Implementation and Impact.** By Charles A. Johnson and Bradley C. Canon. (Washington: Congressional Quarterly Press, 1984. Pp. xii + 288. $10.95, paper.)

This synthesis of research on judicial impact successfully succeeds Stephen L. Wasby's bellwether of 1970, *The Impact of the United States Supreme Court* (Dorsey Press). More, the compendium succeeds on three levels: as introduction for advanced undergraduates, as aid for graduate field examinations, and as assessment for practitioners.

Johnson and Canon recount the results of investigations of four populations whose responses to judicial decisions might be distinct. The findings indicate an interesting interplay of attitudes and behaviors based on general policy and partisan orientations. The interpreting population (primarily lower courts within the jurisdiction of the policymaker) is likely to heed the call of duty and professionalism when issues elicit no intense policy preferences from the interpreters. Outside this assumed "zone of indifference," however, the views of the interpreters are likely to skew interpretations. Similarly, responses among the implementing populations (officials who enforce the policy) are shaped by commitments to existing programs, the desire to conserve resources for agency-defined goals, the preferences of their communities, feedback from targets of enforcement, and continued pressure by courts. Those directly affected by a particular decision (the consumer population) naturally tend to respond according to their perceptions of the costs of compliance and their attitudes. This finding suggests that the zone of indifference is attenuated for the consumer. For the secondary population (a residual category), attitudes toward policies seem to be the major factors in responses to the courts, although the authors admit that research on the secondary population is too scattered to warrant more than speculative conclusions (p. 140).

Interspersed in this distillation of what is known are notations of what is not. Students of the interpreting population have stressed the behaviors of judges, but can only infer attitudes because the direct study of judicial attitudes is so difficult (pp. 48, 66-70). Scholarly attention to the consumption of judicial policies seems to the authors too minimal to support very broad conclusions (p. 108). Among the least understood secondary publics are state legislatures and interest groups (pp. 155-159, 163-170). We learn that there is little evidence of the impact of mass media on the reception and understanding of courts' decisions (p. 175). Extensive survey research has led us to few conclusions regarding the importance of mass and attentive publics' responses to decisions and policies (p. 178).

Johnson and Canon conclude with a discussion of theories in the subfield. The upshot of the discussion is that many theories in the field have not been tested outside of very narrow cases, many theories have not been tested against one another, and indeed, many theories cannot be tested against one another very practically. The authors' efforts to systematize inquiry in the field recall the similarly laudable effort by Wasby in his synthesis to get scholars to speak to theory and to one another. Here Johnson and Canon may have been less willing to depart from the findings of research than some readers would prefer. Wasby admonished practitioners to reexamine their goals and their conceptual and theoretic tools for achieving the goals. Johnson and Canon have nicely synopsized the results of inquiry, but have chosen not to draw inferences for method. They have deftly (and, it seems to me, devastatingly) charted the theoretic progress to date (p. 222), but have not spun out all of the hypotheses and insights implicit in their own framework (which, to be fair, they regard as merely heuristic). For example, they use the "zone of indifference" to make sense of the mix of legal and political responses to judicial decisions in the different populations without pausing to consider how sensible this zone is for each. For the consumer population, the zone of indifference must be very narrow and probably insignificant. In contrast, it may be quite broad for the secondary population. For the interpreting and implementing populations the zone may be variable, and the more reasonable assumption may be a "zone of ambivalence" in which members of the populations are cross-pressured by legal and political attitudes or torn between dissonant values.

So, although some readers may long for what Johnson and Canon did not do and did not promise to provide, this guide to the subfield will reinstill the thirst for theory. On that ground alone this is a worthy contribution to scholarship.

WILLIAM HALTOM

*University of Vermont*

**Mass Media and Elections.** By Richard Joslyn. (Reading, Mass.: Addison Wesley Publishing, 1984. Pp. xii + 313. $15.95, paper.)

This volume fills a gap in the literature on mass media and politics. As is always the case in a growth subfield, at certain points there will be a need for taking stock, synthesis, and appraisal of our knowledge. Joslyn reviews and highlights a broad range of scholarly and practical literature in the field of voting behavior, public opinion, and mass communication in an effort to identify the major ways in which mass media influence presidential elections. This is a tall order indeed given the different disciplines, forms of data, methodologies, and purposes that abound in the media and politics field. No author assaying the task can expect to find a tidy framework that will adequately order our knowledge.

Joslyn posits three themes around which his discussion is organized. The first of these is that the media have transformed the electoral process. The second theme is that it is the interactions among several types of participants (candidates, managers, journalists, citizens) that shape campaign communication. The third of the themes is that electoral communications have demonstrable effects upon voter attitudes and behavior. Obviously such themes have a commonplace quality, but the simplicity is deceptive, as Joslyn's intelligent and judicious treatment of sub-themes and scholarly disputes demonstrates.

Joslyn is eclectic in his approach to evidence, as can be seen best in his chapters on campaign communication, in which he employs excerpts from television spots, strategy memos from consultants, national survey data, an Arbitron media market map, and campaign spending figures. For the most part, Joslyn is able to maintain continuity in his treatment of the diverse material.

The middle portion of the book is a mildly critical overview of how journalists cover campaigns and what the content of that coverage is. Joslyn's notion of how the campaign press corps covers a presidential race seems to be based heavily upon the experiences of 1972 and 1976 (hence several citations of Timothy Crouse's ideas on pack journalism). The 1980 and 1984 elections have already led to a revision of the 1970s' conventional wisdom on this phenomenon, however, and some observers of the process have begun to argue that the self-awareness of the press has led to the demise of the pack approach. This reservation aside, Joslyn does a creditable job of conveying a sense of how American journalists go about the business of deciding what is news.

In the final portion of the volume, Joslyn devotes three substantial chapters to a splendid assessment of the impacts that various forms of campaign communication such as hard news, debates, and television spots have on public opinion and electoral behavior. Although there is nothing new in the way of data in these chapters, they are more than ordinary literature reviews. Joslyn skillfully synthesizes two decades of empirical research, abundant concrete examples from recent elections, and thoughtful interpretations of it all. Anyone needing a good introduction or refresher on the effects literature could hardly do better than *Mass Media and Elections.*

The book concludes with the obligatory chapter on implications for the democratic electoral process. Here Joslyn suggests several possible conceptualizations of elections as media events including (among others) elections as occasions for prospective policy choice and elections as essentially empty rituals of symbolic reassurance.

Apart from the inevitable problems associated with writing a broadly focused synthetic volume, there is another deficiency stemming from the fact that Joslyn has chosen to ignore most of the scholarship on media in sub-presidential elections, except insofar as a specific piece of work might have an idea that is relevant to presidential elections. A chapter explicitly devoted to media in state-wide, congressional, and local elections could have proven to be a useful vehicle for the encouragement of comparative analysis.

This is on the whole a very solid piece of work. It has a good deal of adoption potential in courses on mass media and in courses on the electoral process that allow for a serious consideration of communications. Furthermore, for at least several years, it is likely to be an unusually handy quick reference for scholars needing a concise picture of the state of our knowledge on media and presidential election campaigns.

CHARLES M. TIDMARCH

*Union College*

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**The Political Presidency: Practice of Leadership from Kennedy through Reagan.** By Barbara Kellerman. (New York: Oxford University Press, 1984. Pp. xii + 300. $22.50.)

In the Preface of her book, Kellerman writes that it is not intended to be the definitive analysis of presidential leadership, and she is clearly correct. The study of presidential leadership has taken off in the 1980s, with no fewer than six scholarly titles appearing and two presidential textbooks with a leadership theme coming into print. *The Political Presidency* makes a useful contribution to this burgeoning subject area.

The book is divided into two major parts: The first five chapters are devoted to a theory of presidential leadership; the second section covers the practice of presidential leadership with separate chapters covering the presidents from Kennedy through Reagan. A short epilogue allows Kellerman to tie the work together and to restate her judgment that Lyndon Johnson and Ronald Reagan have been the nation's best political presidents and the most effective directive leaders of the past 25 years.

Although conceding that presidential leadership in America is difficult because of our constitutional system and our anti-authority belief system, Kellerman believes that leadership, or the process of influence between leader and followers, is possible. It is her view that "the political leader who can coax followership through shared rewards stands the greatest chance of success" (p. 32). To be a politically skillful executive, a person must balance both outside societal forces and internal psychological factors in order to achieve stated policy goals.

No psychological factor is as important to Kellerman as the degree to which a president is extroverted. Introverted presidents, and Nixon and Carter are so identified, will find leadership exceedingly difficult in the American cultural context. Extroversion alone is not enough to become a successful leader, as witnessed by Ford, because a president also must display an adeptness at presidential politicking with political elites. For Kellerman, the political president combines an extroverted personality with an inner-directed determination to accomplish his goals.

To test her theory of presidential leadership, the author chooses to examine the major noncrisis domestic agenda items of the six chief executives under consideration. She openly rejects George Edwards's aggregate success level of influence measurement in his *Presidential Influence in Congress* (W.H. Freeman, 1980) in favor of an approach to study a president's attempt at influence on the single issue of greatest importance to him.

Six chapters provide a case study analysis of how presidents exhibited their political skills (or lack of them) in pursuing a key policy objective. The case studies cover Kennedy and federal aid to education, Johnson and the war on poverty, Nixon and the family assistance plan, Ford and the tax cut, Carter and energy, and Reagan and budget cuts. Although Kellerman is precise in explaining her selection criteria of particular policies, one difference shows up between Kellerman's case studies and what Paul Light found in *The President's Agenda* (Johns Hopkins University Press, 1982), where Gerald Ford's primary domestic concern was energy.

In constructing her case studies, Kellerman relies heavily on secondary sources with some archival research. What saves several of these chapters from bordering on the too-often-told tale is Kellerman's inclusion of a concluding section, "The Practice of Leadership," for each president and policy goal. It is in these sections that specific insights into presidential policymaking are made. For example, Reagan's success as a political leader is attributed to his natural political instincts along with his uncanny tactical skill, whereas Carter's failure was rooted in his inability to understand the link between politicking and leadership in American national politics.

The overall value of *The Political Presidency* depends to a large degree on the nature of the reader. Kellerman has explored general psychological leadership theory and applied it effectively to a theory of presidential leadership. The book is of clear value as supplementary reading assignment for courses on American politics and the presidency, but for individuals seeking a theoretical high in the presidential leadership area the recent works of Bert Rockman, Paul Light, and John Kingdom will be more to their liking.

ROBERT LOCANDER

*North Harris County College, Houston*

**Presidential Parties.** By John H. Kessel. (Homewood, Ill.: Dorsey Press, 1984. Pp. ix + 645. $22.95.)

*Presidential Parties* is an excellent analysis of American national politics. John Kessel provides the reader with a unique way of looking at national party activity. Three factors are especially important: the temporal context, internal structure, and external structure.

Kessel explains the ebb and flow of presidential, congressional, nomination, and campaign politics. Each of these has a time component during which certain activities occur sequentially. The temporal factor provides understanding of what presidential candidates, national legislators, and chief executives do within allotted times for action. Most political studies ignore this factor.

The book also provides useful insights on the internal structures of presidential, legislative, nomination, and electoral politics. Kessel applies statistical methods to show how some groups are more complex and cohesive than others. Internal considerations are linked with external factors whereby presidents establish congressional and media liaison, legislators formulate new policy, presidential candidates seek primary votes, and presidential nominees develop campaign staffs and operations.

There are two excellent chapters on the two-candidate campaigns of 1972 and 1976 and the three-candidate contests of 1968 and 1980. Kessel synthesizes nearly all of the available research on the five presidential contests and shows how the various contenders developed strategies and tactics for putting together electoral coalitions.

Kessel is also concerned with how the voters respond to presidential candidates. Over the last three decades, levels of citizen information have remained modest, and strong partisan attachments have declined while the number of independents has increased. The most unusual finding is that issues have been important in all presidential elections since the 1950s. Voters apparently weigh how well the economy is doing (which usually favors Democrats) with international issues (which usually favor Republicans). Other complicating factors are involved in voter choice, but Kessel argues that voters want and need information from presidential candidates. They aren't getting it from television reporting. Even the print media has reduced the volume of issue-oriented stories in recent elections. This trend continued in 1984 when the mass media focused more on the horse race aspects of the campaign.

In a concluding chapter, Kessel effectively draws together the key findings and interrelationships developed earlier.

*Presidential Parties* is a welcome addition to the study of national parties and presidential politics. It is the most effective comparative analysis of recent presidential campaigns and voter response currently available.

ALAN SHANK

*State University of New York at Geneseo*

**Macroeconomics and Micropolitics.** By D. Roderick Kiewiet. (Chicago: University of Chicago Press, 1983. Pp. 176. $15.00, cloth; $5.00, paper.)

For years, political scientists, politicians, and journalists have cavalierly said that "Americans vote their pocketbooks." This generalization appeared to be a happy marriage of political common sense and empirical political science. Indeed, research by Gerald Kramer and others on economic cycles and voting patterns suggested that voters "give greater support to the incumbents during periods of prosperity, but turn against them . . . when times are poor" (p. 6). The 1984 presidential election is an illustration of the pocketbook thesis. Most political scientists and journalists projected a Reagan victory given the state of economic recovery. However, as Kiewiet's work clearly demonstrates, the economic theory of voting, so broadly stated, is too general and ambiguous to account for voter behavior. At the micro level, there are several paths to pocketbook voting.

The basic framework for Kiewiet's analysis is premised on two distinctions. The first is the recognition that voting one's pocketbook can result either from rewarding or punishing incumbents for current economic conditions or can be the result of policy-oriented voting. Policy-oriented voting is based on voters' perceptions of party differences on handling the problems of unemployment and inflation. In the first case, voters lash out at whomever occupies the White House. In the second, voters move toward one party or the other depending upon which economic problem is most pressing. The second distinction is that voters may react to their own economic circumstances or to general economic conditions. Perhaps the most important point that *Macroeconomics and Micropolitics* makes (and documents) is that "it is perceptions of national conditions and events which most heavily influence voting behavior" (p. 21). Kiewiet argues that many voters do not connect their personal economic situation with national trends. These two distinctions produce four types of pocketbook voting: incumbent-personal economic situation, incumbent-general economy, policy-personal, and policy-general.

Kiewiet's basic research strategy is to use panel survey data for the years 1956 to 1960 and 1972 to 1976 to confront the four types of pocketbook voting. The use of survey data is important because many individual behaviors are consistent with the aggregate level findings. The panel data enables Kiewiet to use the respondents' vote in the previous election as a "temporally prior baseline against which to measure changes induced by the particular short-term factors of interest" (p. 28). (Kiewiet also uses cross-sectional survey data for the years in which panel data are not available employing partisan identification as the baseline.)

The basic findings of the study are these: (1) pocketbook (of whatever variety) voting is stronger at the individual level in presidential as opposed to congressional elections; (2) national assessments have greater electoral consequences than personal; and (3) incumbency-oriented voting has a greater effect than policy.

The student of voting behavior thus comes away from this compact volume with the sense that national presidential elections may be considered as referenda on presidential performance with respect to general economic conditions. To this reader, the study would seem to strongly support Morris Fiorina's theory of retrospective voting.

The predictive utility of *Macroeconomics and Micropolitics* was given a test in the 1984 presidential election. While general economic conditions had improved, especially with respect to inflation, unemployment was still relatively high. In the near term, Kiewiet argued that getting inflation down would be the dominant indicator of the national economic conditions. With inflation under control, an incumbent president should have been, and was, reelected. This is one instance where a highly quantitative work of scholarship was right on target.

*Macroeconomics and Micropolitics* would be most appropriate for graduate level courses in voting behavior. It is an exemplar for its sophisticated treatment of survey research.

DAVID H. EVERSON

*Sangamon State University*

**Promises and Performance: Presidential Campaigns as Policy Predictors.** By Michael G. Krukones. (Lanham, Md.: University Press of America, 1984. Pp. 158. $22.75, cloth; $11.50, paper.)

Because of his concern about negative public attitudes toward political campaigns, Michael Krukones has undertaken this study to prove that presidential campaigns should not be regarded as futile exercises during which candidates say one thing and then act in another way once they assume office. In examining 15 campaigns from 1912 to 1976, the author concludes that successful presidential candidates kept approximately 75% of their campaign promises. Thus, campaigns are good indications of the future policy performance of presidents and provide the electorate with accurate information on the policy choices before them.

For each campaign examined, Krukones distinguishes the major campaign issues from other issues that arose in the campaign. He also gives presidents credit for "good faith" efforts to fulfill campaign promises, such as Wilson's efforts to bring the United States into the League of Nations. Finally, he assigns each president an overall percentage rating based on the degree to which the president carried out his campaign promises.

In computing a president's rating, Krukones unfortunately gives all issues equal weight. The problem with such a mathematical analysis of all issues is that voters may recall only major issues. For example, voters may recall that Woodrow Wilson and Franklin Roosevelt promised to keep America out of war, that Lyndon Johnson promised not to enlarge the Vietnam War, that Herbert Hoover promised prosperity, and Richard Nixon promised an "open presidency." Of course, in a democracy, voters have the collective power to unseat an incumbent president if he fails to carry out his campaign promises.

In considering public opinions, Krukones does not take into account that even if a president extends a good faith effort, the voters may believe that "faith without works" is not sufficient to hold the nation's highest office. Although Krukones credits President Carter with a good faith effort to reduce unemployment and inflation, a majority of Americans voted no when in 1980 Ronald Reagan asked, "Are you better off than you were four years ago?"

Krukones admits that presidents do not always keep their promises, and that this may have an adverse effect upon voters. As an example of broken campaign promises, he admits that "the nation had been shaken by the events of Vietnam and Watergate," and, as a result, "trust in government was continuing to decline among the electorate" (p. 109). However, he still gives both Johnson and Nixon high marks for accomplishing their campaign promises (87.5 and 73.5%, respectively).

In the concluding chapter, Krukones factors major campaign issues and good faith efforts into his equation, but some of the results are most amazing. One result gives President Johnson a zero on major foreign policy issues while Richard Nixon receives a 100% rating on domestic policy. It is doubtful that political scientists and historians will agree with these findings.

It is also unfortunate that this text ends with a campaign already eight years out of date. This omission does, however, leave professors and students the opportunity to apply Krukones's theories to recent campaigns. Future studies may improve upon his methods by rating major campaign issues more heavily and not granting "good faith" efforts where they are not warranted.

Even with its limitations in methodology, this brief but well-focused text adds additional insights to one's understanding of the presidency and issues arising in modern campaigns. Although readers may disagree with some of the author's conclusions, most political scientists share his contention that this subject deserves a rational analysis rather than being left to the whims of negative public opinions.

LARRY BOYLE

*George Washington University*

**The Two-Party South.** By Alexander P. Lamis. (New York: Oxford University Press, 1984. Pp. x + 317. $25.00.)

*The Two-Party South* is tightly written around the premise that the politics of race was, and remains, the driving force of southern politics. According to Lamis, the national Democratic party initiatives to further racial equality led to the demise of the Solid South in presidential politics. The initiatives encouraged the somewhat uneven development of competitive Republican parties in the states of the former Confederacy during the late 1960s and early 1970s. In response to the political reality of Republican party growth, the centrality of race abated and led to biracial coalitions producing electoral success for moderate Democratic candidates in the 1970s and 1980s.

Lamis develops the racial politics/competitive two-party politics theme through a skillful and interesting combination of interviews and journalistic election analyses, both of which focus on the state-wide and, to a lesser extent, congressional two-party contests in the individual states. Although presented in each state chapter, incidental to each analysis are the graph depicting the David Composite B index of Democratic party strength and Democratic presidential vote 1932-1982 and a state county map plotting various political/demographic phenomena. For example, some state maps plot 1960 and 1970 census data (South Carolina, Florida, Mississippi, Alabama, and Georgia) rather than those for 1980; presidential (Mississippi, Arkansas, Florida, and Texas) or gubernatorial vote (Louisiana) rather than the first state-level, state-wide Republican electoral success; and simply political regions (North Carolina). Hence, the data presented are uneven, and issues of importance are neglected: Does the 1960 Florida urban horseshoe exist in 1980? Is the balloting for President Ford in Texas more important than the election of a Republican governor in 1978? Rather than note the low percentage of blacks in Texas and Florida, one might want to explore the political importance of Hispanics.

On the other hand, aggregate and limited survey data, although uneven, are brought together in the introductory chapters to supplement the explanation for the emergence and growth of two-party politics from the 1948 Dixiecratic Revolt to the Voting Rights Act of 1965 and its aftermath. The 1960 and 1976 SRC/CPS National Election Studies are used only for partisan identification (p. 36), whereas previously published data for selected cities are used to illustrate the impact of race and class on the 1964-1972 presidential elections. A consistent use of the widely accepted SRC/CPS studies would have strengthened the presentation.

In direct contrast, the concluding chapter, "Southern Politics in the 1980s," rests on an analysis of the 1980 SRC/CPS National Election Study to determine the impact of selected demographic characteristics and political issues on partisan identification and presidential voting. Information is also brought together from individual state surveys, including a secondary analysis of those for Mississippi and Alabama in 1982.

The strength of *The Two-Party South* rests with the skillful use of interviews and journalistic election analyses to examine the impact of race on the development and maintenance of two-party politics in the South. Its weakness lies in Lamis's failure to address the transformation of southern politics through a systematic examination of the impact of the Voting Rights Act of 1965 from the standpoint of voter registration data (presented for Louisiana and Florida only) which are available by race, black electoral participation and success, and Republican party organizational growth beyond federal patronage parties and its success in state legislative and other less visible elections. The reader, then, learns little about the changing dual nature of strong state/weak national Democratic parties in the region.

*The Two-Party South* is interesting, readable, and useful; but it is limited by virtue of its omissions.

CHARLES D. HADLEY

*University of New Orleans*

**Vice-Presidential Power: Advice and Influence in the White House.** By Paul C. Light. (Baltimore: Johns Hopkins University Press, 1984. Pp. 278. $19.95.)

This book's title appears to be a contradiction in terms: What power is there in the vice-presidency? Everyone has long known the weakness of "His Superfluity," as John Adams styled the office. What everyone "knows," however, is no longer true, as Paul Light demonstrates most effectively in this treatment of the contemporary vice-presidency.

Light's thesis is that the second position in American government has emerged from the relative obscurity of past times to become an office of no little substance. As he puts it in his introduction, "After two hundred years as errand-boys, political hitmen, professional mourners, and incidental White House commissioners, Vice-Presidents can now lay claim to regular access to the President and the opportunity to give advice on major decisions" (p. 1). Moreover, as Light makes clear from his analysis of Walter Mondale's tenure and the early days of the Bush vice-presidency, it is an office that also offers its occupant a chance to influence the course of presidential policymaking. Drawing on extensive interviews with current and former presidential and vice-presidential aides, Light develops his thesis through a careful analysis of the job of vice-president, the vice-presidential staff, relations with the president and White House staff, and resources and opportunities for vice-presidential influence.

As the subtitle makes clear, influence is Light's central concern. He focuses on the factors affecting vice-presidential influence over policy and presidential decisions and in doing so provides some useful insights into White House politics. Indeed, the book is as much about the internal politics of the White House as it is about the vice-presidency, with the vice-president holding center stage as a special kind of presidential advisor. Using a comparative case study method to contrast the influence of Vice-Presidents Rockefeller and Mondale, Light reveals how vice-presidents can take advantage of (or be swept away by) the ebb and flow of power in an administration. Consequently, much of what he writes about patterns of advice and influence apply to any senior presidential aide as well as to the vice-presidency.

The book is well-organized, clear, and convincing. Light has done exhaustive research, knows his subject well, and presents his case in an effective and compelling fashion. If there is a flaw to this work, it is that the author is almost guilty of overkill: The book seems almost to be overargued. The word "almost," however, should be stressed. Although there is a great amount of information here about advice and influence (or the lack thereof) by every vice-president since Henry Wallace, there is nothing superfluous in Light's work. His task is to sway the reader from long-held beliefs about the vice-presidency, which is no easy task, so every piece of evidence and every bit of analysis is important to the argument.

Indeed, it would be helpful if the argument could be extended. Before the 1988 election comes around, Light should write a second edition to this book, which would update his analysis by incorporating the lessons and experiences of the

Bush vice-presidency. The comparison between two influential vice-presidents, Mondale and Bush, would be informative. Even without such an addition, however, this is a valuable book.

If, as George Reedy has claimed, the White House is a type of royal court, then Paul Light has written the book of the modern courtier. It should be read not only by students of the presidency and organizational politics, but by prospective vice-presidents, White House aides, and presidents as well.

RYAN J. BARILLEAUX

*University of Texas at El Paso*

**The American Ethos: Public Attitudes toward Capitalism and Democracy.** By Herbert McClosky and John Zaller. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xviii + 342. $27.50.)

*The American Ethos* is a very intelligent and well-written book about fundamental democratic and capitalist values in the United States, and it offers an important and challenging theory about elite influence upon public opinion. The book will not only be of interest to students of public opinion and American politics, but also to political theorists, social psychologists, and historians.

McClosky and Zaller examine mass and elite opinion data from their Opinion and Values (1975 to 1977) and Civil Liberties studies (1978 to 1979) which are supplemented by McClosky's 1958 Political Affiliations and Beliefs study and tabulations (originally compiled by others) from Gallup, Harris, and other surveys over the last 50 years (although very little trend data are examined). Using a large number of survey questions—more than 70 items on democratic and capitalist values from the Opinion and Values study alone—the authors provide a concise analysis of attitudes toward freedom of religion, speech, the press, and assembly; the relatively new issues of moral freedom and unconventional lifestyles; equality of worth, dignity, rights, opportunities, and outcomes; the "Protestant ethic," individualism, ambition, achievement, self-interest, profit, and competition; and property, laissez-faire, and the role of government in the economy.

McClosky and Zaller's conclusion—that there is substantial support for democratic and capitalist values, but also some striking ambivalence—is not new. The ambivalence arises when abstract values are contrasted with political practice, especially in circumstances in which democratic and capitalist values come into conflict. The result is the configuration of attitudes that provides the underpinning for the American welfare state.

What is novel is the authors' theory of public opinion change and the way in which they employ their evidence. They persuasively argue that citizens learn political norms and values from elites—formal leaders and citizens who are active in political organizations, campaigns, and the like—and this is most apparent for those most interested in politics and close to the mainstreams of opinion (p. 11). Their argument is supported by their comparisons of elite opinions with those of citizens with different levels of education and sophistication. Particularly illuminating is the fact that when there is elite consensus (i.e., substantial elite support for a position) on an issue, sophisticated citizens will be much more likely to take on the views of the elites. In the case of increases in political "tolerance," the authors' analysis suggests that it was not political sophistication per se that has fostered tolerance, but that sophisticated citizens have responded to clear and uncontested norms among elites.

The authors' theory is elegant and compelling, but it must compete with other, not necessarily mutually exclusive, explanations. For example, Elisabeth Noelle-Neumann's arguments about the "spiral of silence" are not refuted by their data (e.g., elite opinion may be mistaken by citizens for *public* opinion). Moreover, elite consensus may be less important than the influence of *specific credible elites.* McClosky and Zaller refer to President Nixon's influence upon public opinion toward wage and price controls in 1971 (p. 152), but here there is less evidence for the impact of many elites than for simply effective presidential leadership. As the authors would agree, further study of these competing theories requires comparisons of changes in public opinion with content analyses of the views of different elites over time (p. 317).

If their theory is not on target, the authors' conclusion about the future of the American welfare state may be overly optimistic. They maintain that elite support for the welfare state will be passed on to newer, more politically attentive cohorts. But if the cohorts that have reached young adulthood in the years following the 1978 to 1979 period of the authors' last survey are more responsive than older cohorts to the cues from the Reagan administration and other credible conservative elites, then support for the current welfare state may be more fragile than the authors anticipate.

ROBERT Y. SHAPIRO

*Columbia University*

**Common Cause: Lobbying in the Public Interest.** By Andrew S. McFarland. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. x + 212. $20.00, cloth; $11.95, paper.)

Once a moribund area, the field of interest groups has undergone something of a renaissance recently as new and provocative works have appeared. Drawing on the classic work of David Truman, Raymond Bauer, Ithiel de Sola Pool, Lewis Dexter, and Mancur Olson, research by Terry Moe, John Chubb, Jeffrey Berry, Michael Hayes, and Theodore Lowi, among others, has sought to update earlier interpretations in light of the explosion of group activities that took place in the 1960s and 1970s.

Among these developments, few things have been more controversial than the rise of public interest groups. PIGs, as they often are affectionately called, have been the subject of considerable debate in the popular press. Supporters have defended them as a necessary counterbalance in the contemporary system of interest groups. Given the fact that several forces (i.e., the expansion of the federal role in the 1960s, legal changes in the campaign finance area, and the often-noted decline of political parties) have stimulated private group activities, many observers feel that public interest groups are necessary to limit the demands of greedy and selfish groups.

In contrast, critics have derided public interest groups on several grounds. Despite claims to represent the public interest, writers point to the elitist membership of these organizations (meaning their tendency to attract "upscale" adherents) as well as a more general skepticism about the notion of the public interest to question whether public interest groups really are very different from private interest groups. Others have examined public interest groups and—after finding that many of them lack formal memberships, local chapters, or much means of communicating with their members—conclude that these "groups without constituencies" essentially are front organizations for leaders pursuing their own private agendas.

It is within this context that Andrew McFarland undertook his case study of Common Cause. His research questions are as follows: Who joins Common Cause? Why do they join? What is the structure of the organization? How is it governed? And what has its impact been on the American political scene?

According to McFarland, Common Cause can best be analyzed as part of the tradition of political reform in the United States. Founded by John Gardner, the organization systematically and pragmatically tried to bring about procedural reforms (such as campaign finance and opening up the system) in the policymaking process.

However, the book often is disorganized and unclear in developing its argument. For example, McFarland's discussion of group origins and maintenance wanders from Truman's political disturbance models (i.e., the nature of the times and the need for a public interest organization) to Robert Salisbury's entrepreneurial approaches (the leadership of Gardner) to Jack Walker's patron perspective (the role of outside foundation support) to even Olson's economic approach (the logic of economic sidepayments). The absence of conceptual clarity means that McFarland's conclusions are confusing or give the appearance of a "laundry list" approach to explanation.

In addition, it is not clear exactly how McFarland comes down in his evaluation of Common Cause (either as a public interest group or as part of a reform movement). He concedes that Common Cause has a membership that is predominantly white, professional, and upper-income; that its issues often go beyond good government issues (such as open government and fair processes) to more partisan legislation (such as the B-1 bomber); and that some of its procedures for giving members a role in group decision making are more symbolic than substantive. Yet he also seems to feel that Common Cause has made a major and positive contribution to the American political system (notably in the campaign finance area). It would have improved the book for the author to have better integrated his disparate findings and evaluations.

Finally, I would have liked to have seen more explicit connections with the growing literature on public and private interest groups. McFarland claims that Common Cause is different from other public interest lobbies in its degree of pragmatism, its sensitivity to considerations of internal democracy, and its overt efforts to avoid the mistakes of past reformers. But he does not make these contrasts in such a way as to show how his conclusions fit with the work of others. Since we are experiencing an expansion in the literature on interest groups, McFarland misses an opportunity to make a major theoretical contribution in this area.

DARRELL M. WEST

*Brown University*

**The Politics of Protection: The U.S. Secret Service in the Terrorist Age.** By Philip H. Melanson. (New York: Praeger Publishers, 1984. Pp. ix + 215. $22.95.)

*The Politics of Protection* is a refreshing, long overdue examination of one of the smaller and little understood federal agencies—the Secret Service. By the author's own admission, the book "is the first major analysis of the U.S. Secret Service in 16 years" (p. viii). The central thesis of the book is that the Secret Service is a paradoxical agency—visible, yet secretive; "highly professional and merit based, yet frequently dominated by the political arena in which it operates; a complex, dual-purpose organization with a series of missions and a variety of occupational specializations" (p. viii).

Melanson examines this complexity by giving the book a unique political focus, one that enables the reader to view the interactions of the Secret Service not only in terms of a bureaucratic entity, but also from the point of view of political process and political culture. This approach is well illustrated as Melanson traces the evolving nature of the Secret Service (chap. 1) and adaptable nature of the agency as it interacts with the changing presidential personalities that it is assigned to protect (chap. 7). In essence, the author has opened a new window on the American political process.

Melanson uses methodology that is traditional, descriptive, and analytical. The book is very readable; the study easily bridges the gap between academic and popular interest. Arranged in 11 chapters, it falls into three general conceptual divisions. The first division is descriptive and organizational (chaps. 1 through 4) and includes the historical rise of the Secret Service, its organizational peculiarities, and personnel. The second division describes the complexities of problems the service has to deal with and the increasingly threatening world of contemporary political actors (chaps. 5 and 6). In addition, Melanson depicts the impact of presidential psychological predispositions on agency conduct (chap. 7) and the paramount role of politics in the protection process (chap. 8). The final division involves case study analyses of the John F. Kennedy assassination and the attempted assassination of President Reagan (chaps. 9 and 10). These last chapters are evaluations on how the Secret Service has measured up. The evaluations are objective, with no attempt to gloss over the errors of the Secret Service or to indict the agency unduly with a destructive attack on the agency's character.

If the book has a weakness, it is in the implication of the title. The impression is given that the book will add insight into the growing problems of protecting presidents and combating terrorism in contemporary politics. These problems are discussed, but only in a peripheral manner. Most of Melanson's effort concentrates on history and a discussion of the complexities of protection. The hard core discussion of how to deal with terrorists in any prescriptive sense is absent. This absence may be due as much to the ambivalence of the phenomenon and political culture as it is to the analysis. In the words of former Assistant Secret Service Director James P. Kelly, "It is true that nearly all attempts on the lives of U.S. presidents have been by single gunmen, but the Secret Service's orientation nonetheless is reminiscent of the French General Staff in 1939: prepared to fight the previous war rather than the next" (p. 198).

In some ways Melanson is like the service he studies, dealing more insightfully with the past than giving direction for the future in a terrorist age. It is possible that this task was unrealistic from the outset. Nevertheless, *The Politics of Protection* is a noteworthy effort and a welcome contribution to the literature on American politics and the Secret Service.

JAMES L. WAITE

*Central Missouri State University*

**Clients and Lawyers: Securing the Rights of Disabled Persons.** By Susan M. Olson. (Westport, Conn.: Greenwood Press, 1984. Pp. xv + 236. $29.95.)

Firmly grounded in the research tradition of the late Clement E. Vose, Susan M. Olson's *Clients and Lawyers* is a valuable addition to the scholarly literature on interest group litigation. While perhaps delivering a bit less than the title seems to promise, Olson's book offers a detailed examination of six mid-seventies lawsuits in which interest groups sought the aid of the courts in the battle for public transportation accessible to those with physical disabilities.

The primary contribution of this book is that it substantially extends our knowledge of interest group litigation in the lower state and federal courts, in contrast to Supreme Court-oriented constitutional litigation depicted in the works of such scholars as Vose, Frank Sorauf, and Karen O'Connor. A central theme of the book is the distinction between the "NAACP prototype" of centrally coordinated, lawyer-dominated, precedent-oriented interest group litigation and the "new style" of decentralized, client-controlled, remedy-oriented campaigns illustrated by the accessible transit cases. After introducing this new style of litigation in general terms, Olson presents sharply focused accounts of the six cases (in five metropolitan areas) studied in depth and concludes with an exploration of the implications of the new style.

A significant theme emerging from the case studies is that the accessible transit plaintiffs generally avoided lawyer domination of their lawsuits. Unlike clients in the traditional model of interest group litigation, both interest group and individual plaintiffs were actively involved in making decisions about the conduct of their cases from initiation through settlement to monitoring of consent decrees. Such active client participation, Olson argues, helps avert the danger of relying entirely on the courts in pursuing group goals and improves the prospects of long-term success. Intriguing as these findings are, it should also be noted that differences in client participation may be largely a function of the organizational link between attorney and interest group. Lawyers affiliated with the NAACP's Legal Defense Fund, as well as other civil rights attorneys, were generally activists committed to pursuing group goals through the courts, whereas the lawyers in the accessible transit cases resembled more closely the traditional model of the private attorney retained to represent the interests of clients.

Thus, the most serious problem in the interaction of clients with their activist lawyers simply did not arise in these cases. Civil rights attorneys and lawyers for the poor have regularly faced the problem of what to do when pursuit of the immediate interests of an individual client may conflict with the pursuit of long-range group goals. Because the accessible transit attorneys generally entered the litigation without any prior commitment to specific policy goals, they faced no conflicts when remedy-oriented clients were ready to accept limited gains in preference to pressing for a broader declaration of rights (p. 127).

Perhaps the most challenging aspect of Olson's study is her effort to relate the remedy-oriented strategy adopted in these cases to outcomes. With appropriate caveats, Olson conveys the message that these interest group efforts to achieve social reform through litigation were, given the circumstances, reasonably successful. Yet a careful assessment of gains actually achieved cautions against an unduly optimistic conclusion. In general these suits were resolved by a negotiated settlement or consent decree in which defendant transit authorities agreed not to ignore the needs of disabled riders entirely. In no case did the plaintiffs obtain a decree declaring a statutory duty to provide accessible transit coupled with an order directing compliance. In short, there was no accessible transit decision analogous to either *Brown I* or *Brown II*. Plaintiff assessments of their success two years after the conclusion of their lawsuits were mixed. A rather discouraging picture emerges when such limited gains are examined in the context of a second Reagan term in which budget-cutting proposals seem to place urban mass transit in general—not merely transportation accessible to the physically disabled—at risk.

EDWARD V. HECK

*San Diego State University*

**Regulatory Decision Making: The Virginia State Corporation Commission.** By Laurence J. O'Toole and Robert S. Montjoy. (Charlottesville: University of Virginia Press, 1984. Pp. xiv + 389. $32.50.)

This is a long, discursive, interesting case study. Its subject is the Virginia State Corporation Commission, once perhaps the most esteemed member of what is now an endangered species, the independent regulatory commission. Its focus is organizational change. Its theme is adaptation to political, intellectual, and technological changes in the organization's environment. Its protagonists are the organizational leaders who defined the SCC's strategy, shaped its structure and its organizational processes, and defended its distinctive competencies for over half a century.

As is the case with any good case study, this one enriches our understanding of the complexities of organizational and political life. It reinforces some of our interpretations and challenges others. For example, I have always been inclined to believe that independence matters—that to do good a regulatory commission must have the discretion to do bad, that the social utility of the regulatory commission approach to managing business behavior has been largely sacrificed to the American obsession with due process, and that regulators can be expected to serve the public interest well only where there is agreement as to its nature. I have also been inclined to doubt the utility of life-cycle models of the organization, although not of products or missions. This case study increases my confidence in all of these beliefs. That this has not challenged more of my beliefs is not a reflection on the quality of this work. Rather, it reflects my agreement with the basic premises of the study: that "the decisions of regulatory bodies are affected substantially by their decision-making processes" (p. 6); that choice is affected by the order in which alternatives are considered; the cost of information, and the time available, and that, insofar as they reduce the range of alternatives and consequences considered, so too do the standard operating procedures of the organization in question and the values of its members.

What this work sets out to do, it does very well. Its weaknesses are those of the case study method. But even for a case study, this work is unusually atheoretical. It is, in part, about organizational change, yet it wholly ignores the literature on organizational strategy and structure. It is, in part, about the role played by regulators as agents of the polity, yet it ignores contemporary agency theory (e.g., see Glenn M. MacDonald's excellent review essay in the *Canadian Journal of Economics,* 1984, *17,* 415). Indeed, I am somewhat inclined to fault its authors for methodological parochialism. This is not because they believe that the case study is the best way to study organizational dynamics—it may well be—but because they appear to believe that it is the only way to do so. If so it would explain their failure to acknowledge relevant empirical work such as M. W. Meyer's *Change in Public Bureaucracies* (Cambridge University Press, 1979) or the growing body of experimental studies of organizational and regulatory decision making in the tradition of Charles Plott and V. L. Smith. The authors of this study do acknowledge the influence of Richard Cyert and J. G. March on their thinking, but they ignore their methodology—the formulation of computer simulation models of decision-making routines and the testing of the models' outputs against the past behavior of organizational decision processes. Consequently, they have overlooked a substantial body of work that is directly relevant to their topic, starting with Paul Joskow's articles in the *Bell Journal of Economics* (1972, *3,* 632) and the *Journal of Law and Economics* (1974, *17,* 291).

As a result, this work will be of greatest value to those who are primarily interested in the facts of the case, to those who are interested in state politics in general and Virginia in particular, not to those who are primarily interested in regulatory decision making or organizational change.

FRED THOMPSON

*Columbia University*

**The Politics of Bureaucratic Reform: The Case of the California State Employment Service.** By Michael B. Preston. (Urbana: University of Illinois Press, 1984. Pp. xvii + 193. $16.95.)

Michael Preston's *The Politics of Bureaucratic Reform: The Case of the California State Employment Service* is an extensive and well-documented study of the reform of the California State Employment Service during the 1970s and 1980s. Preston's central theme is that bureaucratic reform may be motivated more by political considerations than by concerns for efficiency. Indeed, he argues that reform is often pursued in order to disorient bureaucracies by "altering the distribution of power and influence in the system" (pp. xiv-xv). The book proceeds to examine the nature of various reform models and strategies and to assess their effect on employment policy in California.

Quite frequently, case studies dealing with social service or labor policy are unable to overcome the difficulty of focusing too narrowly on the case at issue. As a result, such studies may explain a policy event, but they contribute little to an understanding of the substantive policy area in general. On rare occasions, certain case studies have overcome this difficulty and broadened our understanding of the administrative and policy processes. Jeffrey Pressman and Aaron Wildavsky's *Implementation* (University of California Press, 1973) was one such study. *The Politics of Bureaucratic Reform* should enjoy a similar degree of success.

In the Preface, Preston acknowledges that the problem of reform is personally and intellectually significant to him. The book continually reflects his interest in and enthusiasm for the topic. For a number of reasons, this study of the California State Employment Service allows the reform process to be examined extensively. Because the agency was consistently involved in some type of reform effort between 1968 and 1981, Preston is able to benefit from the use of longitudinal analyses in his research. Moreover, the size of the agency (a $700 million budget in 1980 and 1981) certainly qualifies it for consideration as a major bureaucracy under virtually any criterion. Finally, the events described in the book constitute a rich and varied array of reform approaches and experiences. One is even prone to discount the fact that it is just another California case study.

One of the most appealing characteristics of the book is its extensive use of the literature on administrative reform, organizational change, and political power. Throughout the study, Preston skillfully weaves this broader body of literature into case-specific situations, generally giving the event a much broader definition and leaving the reader with a better understanding of its significance. Administrative and policy changes are assessed for their impact on agency personnel and clientele as well as for their impact on the agency itself. The treatment is balanced and fairly exhaustive. At various points, the effects of administrative structure, political leadership, personnel policies, and centralization/decentralization are assessed. As a consequence, the book is a powerful instructional tool that considers both the potential and the limitations of reform. Perhaps just as importantly, Preston avoids overburdening this excellent study with irrelevant minutiae. The book is more than readable; it is interesting.

If there is a weakness in the book, it is the absence of any higher order statistical analysis. The empirical studies used in the book are satisfactory; they are used effectively to substantiate a point or call an issue into question. Still, Preston could have been more innovative in the management of his data bases and more creative in the presentation of his statistical evidence.

In sum, however, *The Politics of Bureaucratic Reform* is a superb case study. The book should very quickly find its way into the mainstream of the literature on administrative reform.

LUTHER F. CARTER

*College of Charleston*

**Forecasting Presidential Elections.** By Steven J. Rosenstone. (New Haven, Conn.: Yale University Press, 1983. Pp. xii + 211. $20.00.)

Whether or not the problem of ecological inference is ever resolved, the analysis of voter decisions and election outcomes will each remain important subjects in the study of American politics. At this point, however, most research is exclusively about voters.

Through a careful analysis of state-level aggregate presidential election returns from 1948 to 1980, *Forecasting Presidential Elections* begins to correct this imbalance. Most of this book is a justification for and test of an econometric model of presidential election outcomes. Included as explanatory variables are changes in real disposable income, incumbency, secular political trends, friends and neighbors effects, "New Deal Social Welfare" and "Racial Liberalism" scales, percent catholic (1960 only), public opinion about the president's mismanagement of war (1952 and 1968 only) and about the party best for prosperity and for peace (1980 only), party factionalism, and candidate issue positions (measured by surveying "scholars of American political parties and elections," p. 174). One can disagree with choices of specific variables (particularly the latter) and with different functional forms, but it is unlikely that these would markedly change Rosenstone's substantive conclusions. Moreover, not only does the book provide an exemplary statistical analysis, but, as is rarely the case in political science, sufficient information is also reported so that the reader can understand precisely what analyses were conducted.

To evaluate the model, two types of forecasts are generated. First are "pre-election forecasts"—based only on information available before election day. Although these forecasts do universally better than those of other political scientists, they never surpass, and are therefore less useful forecasts than, last-day public opinion polls. The second type of forecast used in the book is based on "perfect information." This means, for example, that second-quarter real disposable income was used to forecast the 1980 election even though this indicator was not announced until five days after Carter was defeated (p. 119). These forecasts almost always did better than even the last-day public opinion polls. Thus, what Rosenstone calls "perfect information forecasts" are not really forecasts at all, except in the sense that it was theoretically (although not practically) possible to have perfect information. These "ex post forecasts" are therefore useful in evaluating the model but are useless for forecasting elections. Because the models that perform better than the public opinion polls are derived from information available only after the election, the substantial contribution of this book is to "capture the essential features of presidential contests" (p. 112)—to explain rather than forecast presidential elections. This is a minor difference in practice, but a major difference in conceptualization.

Another point not mentioned in the book is that the model does well even by ignoring most of the factors journalists and politicians attribute to causing presidential election results; these include Ferraro's finances, Ford's "Soviet domination of Eastern Europe," Carter's "ethnic purity," and Reagan's "Are you better off today." It is possible that a "campaign detail" variable that is omitted from the model is correlated with some of the included variables, therefore biasing the results. But although this is statistically possible, it is theoretically unlikely. That Rosenstone does so well in forecasting and explaining presidential elections without these details is confirmation of the suspicions of some (Kathleen A. Frankovic, "The 1984 Election: The Irrelevance of the Campaign," *PS*, 1985, *18*(1), 39-47) that the campaign (or at least most of the rhetoric of which it is composed) is indeed irrelevant.

Considering that in 1980 20% of voters changed their minds in the last few days of the campaign and 10% decided whom to vote for on election day (pp. 28-29), it would have been amazing if Steven Rosenstone were half as successful in forecasting or explaining presidential elections. Well-designed, well-written, and well-executed, *Forecasting Presidential Elections* is an important contribution that should be read by all who are interested in understanding electoral results or voting behavior.

GARY KING

*New York University*

**The Presidency and Public Policy: The Four Arenas of Presidential Power.** By Robert J. Spitzer (University: University of Alabama Press, 1983. Pp. xii + 189. $18.75.)

Challenging the view that presidential power derives from the president's personal political skill, Robert Spitzer calls upon scholars to abandon this Neustadtian (or "great-man") model in favor of a "situational," or policy approach to leadership and power.

There are, Spitzer maintains, "specific characteristics of policies proposed by presidents that shape what the president can do and how well he can do it . . . policies structure the interests involved and help to determine the political arenas in which decisions are contested or made" (p. xiv). In short, different policy arenas produce different political interactions and different success rates.

The policy model that Spitzer proposes is based upon Theodore J. Lowi's "arenas of power" scheme, which suggests that there are four policy areas: distributive, regulatory, redistributive, and constituent. Spitzer attempts to apply this policy model to the presidency.

Using only domestic issues from 1954 to 1974, Spitzer hypothesizes that each policy area produces a different involvement and success rate by presidents, that this holds true regardless of who is president, and that issue area is more important than personal skill in determining success or failure.

Spitzer projects that the level of presidential influence and involvement in policy areas "will be, in order from greatest to least, redistributive, constituent, distributive, and regulatory" (p. 36). Distributive and constituent policies tend to be more consensual, whereas regulatory and redistributive policies are more conflictual. Given these policy domains, Spitzer arrives at "a primary theme of this study: that presidential activities in the four policy areas differ sufficiently to suggest the existence of 'four presidencies'" (p. 38). There is the "special interest presidency" (distributive), the "presidential broker" (regulatory), the "public-interest presidency" (redistributive), and the "administrative presidency" (constituent).

Spitzer attempts to test this policy approach to the presidency by applying both illustrative case studies and congressional voting data to the model. In general, the approach stands the test of congressional voting. The president is able to achieve a higher success rate in the redistributive arena, and the lowest success rate in the regulatory arena.

Does this mean that such factors as the president's political skills, partisanship in Congress, and public pressure do not affect legislation? Although Spitzer does *not* go this far, he does suggest that these forces are less influential than "conventional wisdom" would suggest. "Policy characteristics determine the shape of the president's political universe, at least as it relates to his dealings with Congress" (p. 154).

What lesson can presidents learn from this study? As Spitzer notes, if the main concern is legislative success, they should "concentrate their legislative attentions on constituent and redistributive policy efforts, with perhaps a sprinkling of distributive bills aimed at pacifying particular congressmen and constituents. The high political costs and absence of immediate rewards connected with regulatory policies would discourage any major efforts in this area" (p. 156). The implications of these "lessons" could have a significant impact upon legislation and problem-solving from a national perspective.

This fine study does indeed challenge (although I do not think it will replace) the Neustadtian model of presidential power. An effort to integrate this policy model with such factors as presidential skills, public mood, and partisanship to form a more comprehensive portrait of president-congress relations might give this policy model more credability. On its own, however, I am not sure that the model—although innovative and important—is compelling enough to persuade scholars that it could stand on its own.

Spitzer's study would have benefited from a more explicit application of the policy model to such bursts of presidential legislative success as the early Lyndon Johnson years or the first two years of the Reagan presidency. In spite of these weaknesses, *The Presidency and Public Policy* merits attention and consideration.

MICHAEL A. GENOVESE

*Loyola Marymount University*

**Procedural Structure: Success and Influence in Congress.** By Terry Sullivan. (New York: Praeger Publishers, 1984. Pp. xiv + 284. $29.95.)

In *Procedural Structure,* Terry Sullivan seeks to examine and specify the conditions behind the often-repeated maxim that "procedure makes policy" within the U.S. Congress. This is an important, if arcane, subject, and Sullivan is an expert guide through the familiar, yet extremely complex, terrain of legislative procedure. In the end he gives the reader a valuable atlas for tracking the various paths that a bill may follow, although we remain at some distance from determining how process affects policy.

This is a specialist's book, with two very specific audiences. First, it aims at congressional scholars, for whom it provides both a reference-like discussion of House rules and a set of analyses of how these rules may affect policy outcomes. Sullivan also directs his attention to those who are interested in examining formal relations among political actors, regardless of the institutional setting.

Sullivan succeeds in providing detailed descriptions, often expressed in formal terms, of House rules. His discussions of sequence, resolution, and jurisdiction are very useful; a related discourse—on "resource distribution" rules—is less so. Indeed, resource distribution in the Congress operates differently than do rules that affect resolution or jurisdiction, and Sullivan might well have omitted any discussion of resource distribution within the "rules" rubric.

The book succeeds in presenting the rules as part of a formal set of arrangements that have real implications for the legislative process. For example, Sullivan carefully develops the significance of sequence (pp. 30-34) and later provides some relevant analyses that link sequence to complexity. The manipulation of sequence may be especially significant where "the procedures require that those alternatives which are offered must be of a specific structural type . . . [such as] a substitute rather than a simple amendment" (p. 127). Although all members have some power here, many advantages accrue to a small number of legislators (leaders, Rules Committee members) who have special authority in affecting sequencing. This control has consequences; Sullivan asserts that "the more complex the legislative situation, the more difficult it is for amendments to succeed" (p. 141).

The reasonable inference is drawn that "this research helps one to understand why all the effort put into Rules Committee *activity* is so important" (p. 142). The question remains, however, of whether the Rules Committee members understand fully the ways in which their actions structure the decision-making process. Sullivan provides anecdotal evidence to demonstrate that some, like legislative strategist Richard Bolling, had clear visions of the scenarios that they were scripting. Further anecdotal evidence (not presented here) suggests that other key Rules actors have less acumen in devising procedural ploys. Indeed, the democratization of the House may have opened up the legislative strategist role to a host of members, from both sides of the aisle.

The major limitations of *Procedural Structure* come in Sullivan's attempts to generalize toward conclusions that are broader than the study and its data warrant. A particular problem arises in his analyses of responsiveness within the House. Without any reference he observes that the "objective of a representative legislature is to reflect through its decisions the preferences of those whom it represents. In the long run, failure to realize those preferences is a liability for the institution and for the actors associated with it. Legitimacy in this form, related to the reflection of preferences, will be called *responsiveness*" (p. 84).

Such a working definition oversimplifies a concept that has received a great deal of attention in the recent past. How is the reader to assess a subsequent conclusion that the "failure of to provide responsiveness through its most basic decision rules means the legislature's procedural structure does not merely act as an engine of choice but exercises an independent effect on decision making" (p. 142)? Close observers of the Congress have rarely expected the body's rules to facilitate majority-based decision making. The very number of ways in which the legislature can be responsive to its various constituencies lies at the heart of what makes a representative assembly such a continually intriguing institution to examine.

In sum, *Procedural Structure* is at its best in its specific explication and examination of the rules of the House of Representatives, a most complex organization. As a reference-like volume on the nature of these rules, congressional scholars will find it of great value. The more general analyses of how "procedure makes policy," while suggestive, are only very modest first steps in untying the knot of the legislative process. Where Sullivan sees regularity and pattern across the entire array of legislative activities, I tend to see an unruly tangle, which on occasion can be partially unravelled, but which remains an unlikely candidate for formal and general theorizing.

BURDETT LOOMIS

*University of Kansas*

**Making Bureaucracies Think: The Environmental Impact Statement Strategy of Administrative Reform.** By Serge Taylor. (Stanford, Calif.: Stanford University Press, 1984. Pp. x + 410. $29.50.)

When the National Environmental Protection Act went into effect in 1970, its impact upon agency decision making was never thought to be as tremendous as this volume and other studies have vividly demonstrated. The Environmental Impact Statement (EIS) process, which NEPA requires, has forced development-oriented federal agencies such as the U.S. Forest Service and the Army Corps of Engineers to avoid projects that would damage the environment lest the projects be delayed or even discontinued as a result of court litigation or public criticism. As it turns out, the EIS process has played the role of a 23rd Psalm, according to Richard N. L. Andrews, to which federal development agencies cannot afford to pray if they are to proceed with the blessings of the high priests of the environmental movement.

Writing an environmental impact statement is no easy task; NEPA requires that they must be interdisciplinary in scope and nature with an emphasis on balancing socioeconomic objectives with environmental values. It became imperative for development agencies to add to their staffs a special breed of civil servant such as landscape architects in the Forest Service and archeologists in the Corps of Engineers. These environmental analysts are the personification of the change agents, envisaged by Warren Bennis, Louis C. Gawthrop, and the New Public Administration, who are supposed to play the roles of value allocators and policy advocates in public organizations. Implementation of this innovation, however, was slow and difficult because there was a strong resistance from within the organizations to this kind of social change. The development agencies may have accepted the fact that the environmental analysts are going to be part of their work force permanently, but the analysts will have to deal with the problem of social acceptance by their engineering colleagues in the Corps and the Rangers in the Service. They will also have to rely on the help of the environmental groups to bring pressure upon the agency leadership so that environmental values will be considered in the planning process. As environmental analysts seek the help of such groups as the Sierra Club or the Natural Resources Defense Council, they run the risk of organizational disloyalty. Consequently, they stand to lose their credibility and run the risk of diminishing their effectiveness.

Commenting agencies like the Council on Environmental Quality and the Environmental Protection Agency will undoubtedly reinforce their standing as watchdogs of the environment. But as long as they work in an agency organized along hierarchical lines led by elites who play the game of bureaucratic politics, their EIS analysis will be worthless and meaningless, and the entire process becomes futile.

Environmental Impact Statements can be enriched by adding to NEPA agency offices humanists and ethicists who are better equipped and prepared to address the problem of value allocation. And if these environmental specialists feel strongly that their warnings of environmental damage falls on the deaf ears of agency leadership, there is a way to force agencies to act more responsibly. Under the Civil Service Reform Act of 1978, the way to straighten an agency up is to blow the whistle.

The heart of NEPA is the EIS process. Its objective transcends the environment. Its target is bureaucracy itself and to make it accountable for its acts. In the light of this volume, bureaucracy can listen to advice. It can modify its plans. It can make some accommodations to improve its sagging public image. It may not be the panacea hoped for by students of public administration, but as Frederick Anderson said, it has "led the agency horses to water even if they do not always drink."

A. B. VILLANUEVA

*Western Illinois University*

**Children, Families, and Government: Perspectives on American Social Policy.** Edited by Edward F. Zigler, Sharon Lynn Kagan, and Edgar Klugman. (New York: Cambridge University Press, 1983. Pp. xvii + 460. $37.50.)

Nowhere does the complexity of relations among political and social institutions, social science research, and societal values appear more vividly than in the arena of public policy affecting families and children. This book, conceived in the late 1970s but reflecting the dramatic changes of the early 1980s, encompasses a broad range of these relations. The intent is to provide the reader with a "palette of different perspectives" (p. 4) on the development and implementation of such policies. Twenty-three chapters by social scientists and public and private practitioners give theoretical, political, and historical background, strategic advice, and case study analysis.

The theoretical essays that seem most valuable are related directly to the policy area, such as G. D. Brewer's examination of weaknesses in problem estimation that led to failure to make connection between known data on the rubella epidemic of 1963-1965 and later demands on the service delivery system for the handicapped. Chapters on perspectives in political science and the federal legislative process are not targeted to family policy and are less useful.

A set of essays on federal and state responsibilities in social policy provides an intriguing variety of viewpoints on issues of centralization. J. L. Aber, in a case study of Massachusetts, shows state development of an impressive array of services, with growing provision by the private sector and institutionalized public participation. The effects of rapid contraction on these activities in a state fiscal crisis give a preview of later federal cutbacks. Reagan administration policies are viewed with some optimism by J. E.

Hansen, who looks to a sorting out of government functions and opportunity for more local initiatives. S. B. Heintz, in a study of Connecticut, argues that what Hansen called the "end" of federal support (p. 127) has decentralized costs rather than decision flexibility to state or local officials.

As current trends in the nation and states show, even established programs in this area are vulnerable to reversal. Efforts to influence policy must be equipped with data, learn sophisticated media techniques, and adjust to the shift to the federal budget process as the center of legislative policymaking. One comprehensive essay details difficulties in development and application of childhood social indicators and guides the reader to important sources of data. Research must be protected, even if services decline, or long-range impacts of the decline will be unknown. Other authors consider strategies for effective advocacy.

Eight policy case studies demonstrate complex interactions of research, values, politics, and practice. The search for comprehensive policies with clear goals and measurable results is rejected by most as unrealistic in spite of existing duplication, overlapping, and confusion. The Headstart study, for example, largely attributes the program's success and survivability to its multiple, shifting goals. It is made clear that urgent needs overtake research. Programs cannot wait for comprehensiveness or clarity. The political system itself is recalcitrant to planning, yet as present reverses indicate, in the long run the inefficiencies and lack of measurable gains undercut support. A chapter by Seymour B. Sarenson warns that the narrowness of social science research reinforces a dangerous myopia in application of single policies. His study of implementation of the "mainstreaming" education for the handicapped law shows that without understanding of the system, the intent of the law may be circumvented or other dislocations occur. It is just the breadth Sarenson advocates that others view as impossible.

The case studies also show ambivalent attitudes to the role of values in family policy. Those who support government action speak from convictions of important values. Their very claim for government actions, however, often phrased as "intervention," grates against other values of privacy and market primacy. Moral convictions of those supporting or opposing government policies may add intensity to their efforts. Yet as in child abuse and drug abuse remediation, the moral overtones may interfere with effective treatment. Value conflicts and distaste for righteous policies of any stripe form the context in which child and family policies must be forged. The difficulty is to devise programs that will meet needs and endure through the inevitable shifts of dominant values in a society with mixed loyalties.

This book is indeed a palette with resources for scholars, professionals, advocates, and political leaders. The approach is nonpolemical, scholarly, and displays keen sensitivity to the political process. A more incisive discussion of the issues raised by the chapters would enhance its usefulness. The introductory preview is superficial and not fully accurate as it deals with coming chapters. The stated purpose, however, is achieved on a level that is both encouraging and helpful to those who continue to study and work in the fields of human services.

MARY ANNE CAVICCHI

*John Carroll University*

## Comparative and Other Area Studies

**Political Anthropology Yearbook:** Vol. 2, **Culture and Political Change.** Edited by Myron J. Aronoff. (New Brunswick, N.J.: Transaction Books, 1983. Pp. 221. $29.95, cloth; $12.95, paper.)

**Political Anthropology Yearbook:** Vol. 3, **Religion and Politics.** Edited by Myron J. Aronoff. (New Brunswick, N.J.: Transaction Books, 1984. Pp. 139. $29.95, cloth; $12.95, paper.)

The term *political anthropology* usually refers to the study of political processes in the types of societies that anthropologists study—that is, in societies that may not have a clearly demarcated political domain. Political anthropologists have thus faced in its most acute form the problem that political theorists face in trying to define "politics." With recent interest in interpretive anthropology (in the manner of Clifford Geertz) and the study of symbolism, the cross-cultural applicability of a Western term like *politics* has come to seem even more problematic. These volumes on the relationship between politics, culture, and religion speak directly to such difficulties.

As is usual with collections, some of the essays of these volumes are quite good, whereas others are pedestrian. Volume 2 focuses on the contrast between culture and ideology, that is (as Aronoff defines the distinction), implicit patterns of meaning relatively unaffected by rational reformulations versus explicit, rationally formulated beliefs that can be self-consciously affirmed or attacked. Aronoff and several of the contributors think of myth and ritual as examples of culture and rational discussions of political means and ends as exemplifying ideology. They tend to agree that implicit cultural patterns constrain political action, keeping it within the confines of a status quo, to a greater degree than ideology. This is because change comes about when cultural values are explicitly examined (as in ideology), then renewed, reinterpreted, or rejected. All the essays of Volume 2 speak more or less directly to these issues. Particularly successful are Marc Swartz's discussion of domestic politics in the Swahili nuclear family, W. Lance Bennett's analysis of American elections as nonrational rituals, and (in a more ethnographic mode) James Scott's depiction of common sense and political criticism in a Malay village.

There are several subtle but important criticisms to be made of the culture/ideology distinction as Aronoff and some of his contributors (particularly Bennett, David Kertzer, and Mary Hegland) have formulated it. First, they overlook the possibility of a nonrational basis of rationality, if I may so phrase the matter. In other words, they assume that rationality can be easily defined, or at least that it represents an unproblematic dimension of human thought. But many linguists and anthropologists would challenge that notion, arguing that *all* thought depends upon implicit linguistic and/or cultural patterns. In this view, Aronoff's concept of rationality suggests the hubris of the Western tradition, which has typically seen its own—culturally specific?—rationality as universal, as rationality writ large. If such a critique has merit, we would have to revise Aronoff's theoretical formulation to show that a political status quo might equally well endure on either rational or mythic foundations. And that suggests that his notion of myth might need to be revised as well, for it may not be as easily distinguished from rationality as he and some of his contributors suggest. (To what degree is the notion of rationality our myth?)

Volume 3, on religion and politics, is less coherent than Volume 2 because the collected essays do not engage the fundamental issue raised by I. L. Horowitz in his preliminary comment to the volume. Horowitz asks us to reevaluate our common-sense understanding of the categorical distinction between religion and politics and suggests that to the degree that we fail to do so, we may be condemned to misunderstand social action in non-Western contexts where religion and politics may not be separate or (I would add) relevant categories. Unfortunately, the essays in Volume 3 do not explicitly pursue such questions. Two essays on religion and politics in Israel (one by Aronoff, the other by Charles Leibman and Eliezer Don-Yehiya) would seem to belong in Volume 2, for they focus on the reformulation of religious ideology for political as well as religious goals. Essays on Iran (by Jerrold Green) and Lebanon (by Augustus Norton) confine themselves to the role of religious groups in political maneuvering without using the data to reflect on the larger issue of whether religion and politics are helpful scientific categories for those cases. Indeed, one might go beyond that question to ask about political anthropology itself—for there is an uneasy relationship in these essays between political science and anthropology. Many of the analyses of ideology and culture would be read by anthropologists as "anthropology" pure and simple rather than "political anthropology." (I should mention in this regard the fine piece of intellectual history found in Wilson Carey McWilliams's contribution to Volume 3, "The Bible in the American Political Tradition.") Other essays seem to me (an anthropologist) to be straight political science. Is that difference (between the two groups of essays) related to a more fundamental distinction between interpretive approaches versus positivistic ones, symbolic versus social-structural analysis? Would a radically interpretive approach throw out *politics, religion,* and even *social structure* as Western terms that cannot be cross-culturally applied? Such are the questions raised by these volumes and by the type of interdisciplinary inquiry that Aronoff is to be commended for organizing.

RICHARD HANDLER

*Lake Forest College*

**Scheming for the Poor: The Politics of Redistribution in Latin America.** By William Ascher. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 348. $25.00.)

A generation ago in *Journeys toward Progress* (Twentieth Century Fund, 1963), Albert O. Hirschman argued the case for "reformmongering," or the potential for the contriving of reform by astute policymakers within the confines of the seemingly resistant economic and political structures of Latin American countries. However, the ultimate failure of the reforms Hirschman touted, the increasing militarization of Latin American politics beginning in the mid-1960s, and a growing preoccupation with "revolution" called Hirschman's reformist optimism into question and made his analysis seem less relevant to Latin American political realities.

*Scheming for the Poor* by William Ascher revives such concerns. In a vein rather similar to Hirschman's, Ascher's central point is that:

> In case after case the masters of redistribution prove to be the tacticians rather than the warriors. The best records of redistribution are held by the pragmatic politicians whose familiarity with the policy process enables them to manipulate the political atmosphere to lull, disarm, or intimidate the . . . potential allies among other groups fearing drastic redistribution. (pp. 18-19)

The heart of the book comprises a series of case studies of postwar regimes in Argentina, Chile, and Peru and is divided into sections that examine authoritarian populists (Perón, Ibáñez, Odría), democratic reformists (Frondizi, Illia, Frei, and Belaunde), and radicals (Allende and Velasco). Conclusions are drawn concerning the reasons for the successes and failures of the "redistributional" policies of each regime, and each variety of regime as well as redistributional policymaking in general. Although Ascher is certainly aware of contextual and structural constraints, his interpretation of the evidence is broadly nondeterministic, affording substantial scope for leadership and redistributional change within Latin American policy processes under a variety of regime types.

In a sense the book can be read as a primer for policymakers. It is replete with such generalizations/admonitions as: progressive policy measures tend to generate additional demands, rather than increased support, from beneficiaries; the beneficiaries of redistribution tend to resist the extension of benefits to others; allies from the non-poor are essential to redistributional success; and improvisation and attention to detail are important for policy success even on the part of radicals with initially sweeping programs.

Critics may wonder how significant or permanent the redistributional policies of Ascher's regimes may be, given that many of them entail such things as changes in tax rates (which Ascher argues often have a more effective redistributional impact than more dramatic changes) rather than deep structural transformation. Given the ultimate fate of, say, Hirschman's Colombian agrarian reform law of 1961, so apparently impressive in its legislative phase but extremely modest in its implementation, one may indeed wonder whether truly significant redistribution can take place in Latin American political systems short of something like the Cuban Revolution. Some of Ascher's conclusions concerning the treatment of beneficiaries may also miss the political realities of the need to cultivate one's initial supporters merely in order to remain in power.

Still, *Scheming for the Poor* is clearly a major contribution to the slowly growing literature on policymaking in Latin America and is perhaps the most significant contribution to date to the literature on comparative public policy from an author writing on Latin America. The book has important normative implications as well. It will give sustenance to those who hold that meaningful change on behalf of the poor can be attained without full-scale revolution, and that the fate of regimes and their policies owes as much or more to political leadership as it does to determinants of economics or social structure.

ROBERT H. DIX

*Rice University*

**The Industrial Future of the Pacific Basin.** Edited by Roger Benjamin and Robert T. Kudrle. (Boulder, Colo.: Westview Press, 1984. Pp. xvi + 285. $27.50.)

The subject of this edited volume represents one of the more fascinating and important topics in the contemporary world political economy. The volume examines the substantial shifts in industrial production and foreign trade patterns among countries on both sides of the Pacific. Contributors include government officials and academics from Japan, the Philippines, South Korea, Thailand, and the United States as well as representatives from the Asian Development Bank and the United Nations. Primary attention is given to the newly industrializing countries (NICs) of Asia (Hong Kong, Singapore, South Korea, Taiwan), although consideration is also given to other developing countries in the region (notably, the Philippines) and the Pacific's more advanced industrialized countries (Japan and the United States). This region is characterized by perhaps the most dynamic changes in economic growth, industrial development, comparative advantage, and export trade in the entire world economy. Such dynamism demands explanation. In particular, the identification of the sources of such industrial growth and changing comparative advantage stands out as the most compelling issue in this area of study.

Part 1 offers qualitative and quantitative descriptions of the rapid industrial and foreign trade changes in the region. The causal link from industrial restructuring to aggregate economic growth is documented. Ventures beyond this important and basic point, however, flounder. Policy recommendations are made without sufficient analysis. The NICs, we are told, must foster further industrial adjustment to avoid underspecified economic problems (e.g., unbalanced growth). Why we should be concerned about the growing pains of the NICs rather than the problems posed *by* the NICs for the mature industrial economies is not established. Recommendations are made to avoid collision among rival industrial economies. For instance, the Southeast Asian economies should concentrate on labor-intensive industries while the NICs move on to more technology-intensive sectors. However valid such advice may be, it remains vacuous without an understanding of the motive forces underlying industrial change in Asia. Only after understanding why the Pacific economies are riding each others' bumpers in a race for industrial development can we consider sound measures to coordinate economic relations. More importantly, initial efforts to identify these motive forces become muddled. After recognizing the importance of structural economic change for aggregate growth, the causal arrows are reversed when it is argued that economic development can explain changes in comparative advantage and export structure. While feedback loops are to be acknowledged, tautology must be avoided.

Greater success is made in Parts 2 and 3, where three candidates are considered as accounting for Asian industrial development: government industrial policies, labor force characteristics, and foreign direct investment. Other than covering well-known ground, a chapter on Japanese industrial policy suffers from two flaws. First, after reviewing the record of government support of industrial development and changes in Japan's industrial structure, the critical link between the two is established with neither qualitative nor quantitative analysis. Second, the relevance of the Japanese case for structural change in the industrializing "new Japans" is not made explicit. The causal link between government policy and industrial restructuring is much more carefully explored in a chapter on the Philippines. A shift away from two decades of import substitution provided opportunities for Philippine industrialization and export expansion in manufactured goods in the 1970s.

Another possible source for industrial change in Asia is considered in a chapter on labor force characteristics. In particular, a strong correlation is established between the educational attainment of a country's labor force and the composition of the country's industry. This relationship deserves further investigation because the authors, Andrew Mason, Sung-Yeal Koo, and Wi-Sup Song, admit the correlation may be spurious and do not attempt to unravel the direction of causation between high levels of education and high levels of development.

Although three separate chapters are devoted to the role of foreign direct investment in Asia, only one explicitly considers the multinational corporation (MNC) as an agent of structural adjustment. One chapter explores the proclivities of MNCs in South Korea with regard to industrial concentration (higher for MNCs than domestic firms), capital and technology intensity (higher), labor productivity (higher), and profitability (no difference). Although such findings would certainly provide the basis for further empirical investigation, the characteristics of foreign capital are not, in turn, linked to broader changes in the Korean industrial economy. A second chapter treats foreign direct investment as a dependent variable to be explained rather than an independent variable with an impact on structural change. Indeed, both of these chapters observe that the pattern of foreign direct investment in Asia may *reflect* rather than shape country comparative advantage. Whether foreign investment follows or leads comparative advantage is a critical issue deserving more empirical research. Only a third chapter (chap. 9) hypothesizes that industrial restructuring can be brought about by foreign direct investment. The diffusion of technology by the multinational is identified as a major agent of change.

In an otherwise comprehensive survey of the Pacific basin economies, the absence of any serious treatment of China is notable. In addition, reference is made throughout the volume to the fostering of a "Pacific basin community" without any explication of the rationale for the coordination, let alone integration, of the respective economies.

Important issues and provocative hypotheses emerge from this volume. The contributions highlight the need to sharpen those issues and further investigate the hypotheses.

GLENN R. FONG

*University of Illinois at Chicago*

**Change in British Politics.** Edited by Hugh Berrington. (London: Frank Cass, 1984. Pp. 239. $29.50.)

Hugh Berrington and his 10 fellow contributors have produced a very useful collection of articles on British politics in the Thatcher years. The major themes concern electoral realignment and changes in the party system, but regionalism and local government are also included along with informative papers on both business and trade unions.

The 1983 election results are sketched by Michael Goldsmith and Ken Newton, placed in the context of postwar partisan dealignment (the loss of partisan support for the two major parties) by Ivor Crewe, and interpreted in terms of the growth of regionalism by W. L. Miller. The rise of center politics with the formation of the SDP-Liberal Alliance is documented by David Denver, and James Douglas and Philip Williams consider changes in the Conservative and Labour parties respectively. This arrangement of discussion leads to some repetition, particularly in the lists of electoral swings and party supports, but Berrington's editorial control is much more evident than is the case in some collections of articles. This is not to say that the conclusions reached show much more consensus than has marked the period in British political life under scrutiny. For example, the section on regionalism portrays the Thatcher government as destroying local democracy with an overbearing centralism, whereas the chapter on the Conservative party, although critical of the ideological fervor of the first years of the Thatcher administration, argues that a more moderate, incrementalist approach is now visible.

Most contributors stress the economic impact of Britain's decline as a manufacturing nation. Higher unemployment, the differing regional economic performances, the decline in the traditional class solidaristic basis of the Labour party, a new professionalism in both party workers and activists, the failure of attempts at corporatist economic control, and the failures of both Keynesian and monetarist economic policies all have resulted in a political environment markedly different from the cozy complacency of the Macmillan years. The electorate has come to terms with greatly reduced expectations but has lost faith in party politics. The new generation of British voters lacks class loyalty and is regionally diverse and volatile. The old landscape of British politics has been partly destroyed, and students of British politics await the erection of a new architecture on the site with very open minds about the likely design. Party activists, particularly in the Labour party however, are increasingly certain about their differing political truths. Williams's analysis is interesting here. He examines the rise of the Left amid the fall of the Labour party and compares the new Labour militants with the revolutionary determinists predominant in some Continental socialist parties in the 1920s.

The problems of the Conservative party, on the other hand, have been disguised by their landslide victory in 1983. Despite a lower share of the poll than in some previous defeats, they now dominate the Parliament due to the split in the Opposition and the vagaries of the electoral system. And the electoral system could crush them if their support was slightly reduced. This is important because Conservative support depended heavily on the temporary surge of imperialist sentiment that followed the Falklands affair in 1982. The Conservative party is now a regional party largely confined to the South and Midlands. Its parliamentarians are thus largely removed from the experience of economic decline in the North and Scotland. Desperate industrial strife and renewed demands for separation in Scotland are likely developments. The party of national unity and industrialism has shed its patrician style of leadership and now attempts to impose centralist policies on a disunited kingdom. Berrington and his fellow authors have raised these important paradoxes in our assumptions about modern British politics. There remains much theorizing to be done if these paradoxes are to be resolved.

JEFF ARCHER

*University of New England (Australia)*

**Coalition Government in Western Europe.** Edited by Vernon Bogdanor. (Portsmouth, N.H.: Heinemen Educational Books, 1984. Pp. vi + 282. $42.00.)

In the early 1980s the British went through a crisis of confidence in two-party government. The polls were suggesting that the alliance between the new Social Democratic party and the Liberal party could prevent either the Conservatives or Labour from winning control of Parliament. Holding the balance of power, the alliance would then demand the installation of proportional representation, thus making coalition government a permanent fixture of British politics. This book is a result of that period. It is the product of an international seminar held in October, 1982 at the Policy Studies Institute in which experts from a number of Continental countries explained how coalition politics worked in their nations so that the British could be prepared to cope if the need arose.

Bogdanor provides erudite introductory and concluding chapters that eschew formal mathematical coalition theory in favor of a cautious "lessons for Britain" analytical viewpoint. He asserts that coalition government is more than a modification of the Westminster model of parliamentary practice. It is a specific type of government whose rules and conventions all flow from the fundamental principle of power sharing. Bogdanor wants to investigate such questions as how the role of the monarch differs in countries where coalition building is the norm; how coalitions alter traditional British notions about cabinet responsibility; whether they strengthen or weaken parliament with regard to the executive; and how coalitions alter the functions of elections in the political system. Do they make any difference in the system's ability to make policy or in the content of policy outputs? Bogdanor effectively illustrates the complexity of trying to answer these questions definitively. For the U.K., he writes, those who would favor coalition government do so because they believe "that a basic consensus in British society is frustrated by the adversary process in Westminster. A coalition, on this view, will be better placed to mobilize consensus in the interests of social and economic modernization" (p. 277). But, ever cautious, he is not ready to claim that this book demonstrates the truth of that position. Instead, he calls for more research to identify the conditions under which coalition government is likely to lead to consensus and progress and those under which it will result in immobilism.

The 10 country chapters offer the usual mixed bag. West Germany, Scandanavia, and the Netherlands get two chapters apiece. The first set of chapters (by Klaus von Beyme, Pertii Pesonsen and Alastair Thomas, and Jan Vis) describes the politics of coalition formation. The second set (by Manfred Schmidt, Bo Sarlvik, and Ken Gladdish) assesses the impact of coalitions on policy outputs. The chapter written by Geoffrey Pridham on Italy adequately covers the political side but totally ignores policy outputs. The chapters on Ireland and Belgium are too brief to be of much interest to readers with some background knowledge of these countries. Finally, there is an exploratory chapter by Collin Mellors on coalition strategies in British local governments based on the experience of 83 "hung councils" during 1981 and 1982.

What does it all add up to? We know, of course, that in the parliamentary elections of 1983 the Tories won 61% of the seats with only 42% of the votes. So the book's original thesis is not as timely as it once was. There is no overarching theoretical framework to unite the diverse chapters. Bogdanor's chapters comparing the way parliamentary government works in Britain and on the continent are interesting but hardly earthshaking. Some countries (France, Switzerland, Austria) that have interesting variants on coalition politics have been left out entirely. In America, this book will primarily be of use to specialists on parties in West Germany, Scandanavia, or the Netherlands who will find that the relevant sections offer a helpful synoptic overview coalition government in these three nations.

JAMES A. DUNN, JR.

*Rutgers University, Camden*

**Constitutional Reform and Apartheid: Legitimacy, Consociationalism and Control in South Africa.** By L. J. Boulle. (New York: St. Martin's Press, 1984. Pp. xii + 270. $27.50.)

In 1984 the Republic of South Africa adopted a new constitution that, for the first time, established parliamentary representation for its Coloured and Indian population. Boulle, a South African law professor, examines the historical process behind these constitutional changes and analyzes how the apartheid government has invoked theories of pluralism and consociationalism to justify these new structural alterations.

Boulle begins his study with a review of modern Western constitutionalism, focusing on the Westminister parliamentary system and the American presidential system. He notes that both systems are premised on the principle of majority rule and an adversarial political context, and that each system includes constitutional checks and balances to prevent majority rule from becoming unrestrained.

Boulle's second chapter explores several theories of pluralism that have emerged in Britain and America and concludes that the majoritarian assumptions inherent in these traditions are ill-suited to the present South African government because it presumably will result in permanent political majorities and permanent minorities. Chapter 3 explains that consociationalism has been introduced in South Africa as an alternative to the British and American systems because it provides an opportunity for all groups to be represented in decision-making processes. South Africa's new tricameral parliament represents the reification of this consociational alternative, allowing whites, Coloureds, and Indians their own institutional framework for advancing their respective interests.

Chapters 4 and 5 provide an historical overview of the main constitutional developments in South Africa since it became a union 75 years ago. The constitutional policies of the ruling National party are contrasted with other political parties in South Africa including Inkatha, a Zulu national movement. Boulle also provides a comparative analysis of South African constitutional developments with constitutional experiments in Namibia; Rhodesia-Zimbabwe; and the quasi-independent African "homelands" of Transkei, Ciskei, and Bophuthatswana. In addition, he describes the constitutional debates of several recent commissions that have influenced the rejection of unqualified majority rule in South Africa.

Boulle uses his book to join several other scholars who have questioned the feasibility of the consociational option in South Africa. Boulle notes that there is no voluntary group membership in South Africa, there is no multiple balance of power among existing groups, there are few overlapping memberships among groups, there is no strong tradition of political moderation and cooperation, the country is fairly large in size and population, it has a high international profile, the external threats against it are not uniformly perceived by all inhabitants, and there is a high degree of socioeconomic inequality (p. 65). Not surprisingly, the new constitution falls far short of the consociational ideal, resulting instead in an enormous concentration of power in the state president (and his cabinet), who possesses "sovereign legislative authority" and is immune from any judicial review process. A vote of no confidence by the three houses of Parliament could encourage the state president's resignation, but the new constitution provides the president with the legal option in this circumstance to dissolve Parliament. Boulle reports that even Arend Lijphart, the political theorist most closely identified with consociational theory, has described South Africa's form of consociationalism as a travesty of his conception of the system (p. 185).

In short, Boulle has written a comprehensive, scholarly, and convincing indictment of the constitutional changes that are taking place in South Africa. He criticizes the omission of blacks (who constitute the great majority of South Africans) from the constitutional process, arguing that this precludes the political inclusiveness that consociationalism presupposes to stabilize the system (p. 217). Given the crisis of legitimacy facing South Africa today, one must also admire Boulle's courage in admonishing his government's entrenchment of the apartheid system.

ALLAN D. COOPER

*St. Augustine's College*

**Grüne Politik: Ideologische Zyklen, Wähler und Parteiensystem.** By Wilhelm P. Bürklin. (Opladen: Westdeutscher Verlag, 1984. Pp. xiv + 270. DM 37.)

In recent years the pattern of party stability and frozen cleavages in most Western democracies has been supplanted by party fractionalization and increasing electoral volatility. One of the most interesting and significant examples of these trends is West Germany, where the established parties are now challenged by the emergence of the strongest ecologist party in Western Europe, *Die Grünen.* The Greens have attracted the attention of many scholars, but their true meaning is much debated. Some argue that the Greens signify a revival of German romanticism, the youth movements of the 1920s *(Wandervogel),* a conservative protest against too rapid social change, or the new political cleavages of postindustrial societies. Bürklin's study is a systematic attempt to identify the social and political meaning of the Green movement in West Germany.

*Grüne Politik* is the first major empirical study of popular support for the Green party. Most of the book is based on a series of public opinion studies conducted from 1976 to 1980 as the ecological movement coalesced into the Green party. Additional data are drawn from a survey of Green candidates in the 1980 state elections in Baden-Württemberg and several other opinion surveys conducted by the researchers in Mannheim. Separate from the theoretical interests of the author, this book contains a wealth of information for students of West German public opinion.

Bürklin finds that the popularity of the Greens exceeds their early, or even present, support at the polls. As others have shown, this popular support is heavily concentrated among the young and better educated. However, Bürklin makes an important extension to these findings by demonstrating that the political characteristics that define Green support have a much stronger effect for younger generations. Bürklin cites this consistent pattern as evidence of a generational change in the bases of political conflict in West Germany, a point that has relevance for many other Western democracies.

Another component of the study examines the civic attitudes of West Germans—such as political trust, system support, political efficacy, and acceptance of conflict—and their relationship with Green support. Several political analysts have labeled the Greens as a radical, antidemocratic party. The empirical evidence is more mixed. Green supporters are less positive toward the political system and established political institutions, but at the same time they accept democratic political norms and have high levels of political efficacy. Bürklin reconciles this apparent contradiction by suggesting that Green voters have idealistic views that contrast with political reality. In other words, the Greens may reflect the same type of "creedal passion" that Huntington has described in the American context, and the good and bad consequences that this implies.

This study provides a rich blend of political theory and empirical data. In one area, however, the theoretical framework may be overextended. Bürklin describes the Green phenomenon in terms of a cyclical model of political change, largely derived from American realignment theory. Young West German voters represent the "third generation" following a realignment and are therefore available for a new basis of political mobilization. This model has great heuristic value, but it can be applied too rigorously; political change does not follow a predictable sine curve, and even a general pattern of idealistic/reactionary conflict overlooks important differences in the specific issues of conflict.

The Green party represents an important example of the political changes now affecting Western democracies, and Bürklin has written a study that deserves a wider audience than just German specialists.

RUSSELL J. DALTON

*Florida State University*

**The British General Election of 1983.** By David Butler and Dennis Kavanagh. (New York: St. Martin's Press, 1984. Pp. xii + 388. $29.95.)

This book is the twelfth in the series, beginning in 1945, of British general election studies sponsored by Nuffield College, Oxford. The authors acknowledge an "obligation to provide material in a form comparable to previous studies" (p. xi). Their study follows a now-familiar format. Individual chapters survey political events since the preceding election; report developments within each of the major parties; follow the activities of the national party leadership during the campaign; analyze opinion polls, the broadcast media, and the print media; and anecdotally discuss candidate selection and electioneering in the constituencies. One appendix reports constituency electoral results; another provides aggregate psephological analysis of the results.

The 1983 election study, like its predecessors, is a successful synthesis of political science and contemporary political history. Its intention is much less explanatory or interpretive—although such material is artfully woven into the discussion—than it is descriptive. Butler and Kavanagh present a variety of interesting insights and statistical descriptions of specific electoral topics rather than a sustained argument or theoretical model. Their observations are invariably careful not to stray beyond the limits of the evidence or of good sense. In keeping with its concept, the book places more emphasis on short-term rather than long-term electoral influences, and more on the impact of personalities and events than on the consequences of social and economic forces. This is not a criticism, for the authors do very well what they intend to do. The study admirably recreates the experience of the election, which can be appreciated at two levels—among both a general audience and a specialist readership.

If the book has a thesis, it is that the 1983 election was not so much a Conservative party victory as it was a Labour party defeat. Butler and Kavanagh identify three possible reasons for Labour's electoral rejection: (1) the party's poor performance during the campaign, (2) its betrayal of its socialist principles, and (3) its loss of credibility over time from immoderate policy positions and perceived incompetent leadership. They explicitly reject the first explanation—raising questions about the significance of the very phenomenon that their monograph analyzes in detail—and clearly support the third in favor of the second. This explanation is consistent with the findings of electoral research of Crewe and others concerning the long-term decline of Labour issue-partisanship and partisan identification in the British electorate. However, such an explanation makes it difficult for us to understand Labour's lead in public opinion polls from the third quarter of 1979 to the third quarter of 1981. Perhaps polls of voting intention do not elicit valid responses unless an election appears imminent. In any case, an implication of their view is that the "Falklands Factor" or the "Thatcher Factor" were relatively less important to the election than conventionally thought.

Much of the analysis in the *British General Election of 1983* rests upon aggregate survey data and electoral results. A definitive understanding of the election, of course, must await individual-level investigation. But if not definitive, this work is certainly authoritative. Its conclusions shape the received wisdom on the subject. In fact, the Nuffield studies constitute in themselves an important British electoral institution. This institution may actually outlive such other "classical" features of the British polity as its two-party system or its plurality electoral system. (One wonders what Bagehot might have revealed about these studies had *The English Constitution* been written in our day.) Regardless, the Nuffield studies are a singular political institution requiring virtually no reform.

JEFFREY B. FREYMAN

*Transylvania University*

**Absent Mandate: The Politics of Discontent in Canada.** By Harold D. Clarke et al. (Toronto: Gage Publishing, 1984. Pp. xi + 193. $11.95, paper.)

In 1974, 1979, and 1980 general elections were held in Canada, the last two within a period of eight months. This book is an analysis of Canadian electoral behavior on those occasions undertaken within the larger context supplied by public opinion data from panel studies conducted during the preceding decade and from other sources.

The thesis is stated summarily on page 174: "The political issues associated with the economic crisis of the 1970s and 1980s in Canada arose in a political context unequipped to deal with them." Because governing parties in Canada seldom gain a majority of total votes and gain what votes they do in regional concentration, there is no national mandate; because victory is independent of sustained policy preference, there is no "directional" mandate; because voters' choices indicate conflicting and short-term preferences, there is no policy mandate. Accordingly, the only mandate that can be said to exist is what the authors call a performance mandate; that is, elections empower the government to do something, without specifying what is to be done. When governments fail to perform, as according to the authors they have failed, public discontent increases. Hence the book's title and subtitle.

There are useful figures provided regarding shifts in public opinion during the 1970s, but the argument as a whole resembles nothing so much as a suit constructed by a committee of Laputian tailors.

Stated in commonsensical terms, sophisticated analysis of public opinion data shows that support for long-term change, which the authors seem to think desirable, cannot follow from elections fought by "brokerage parties" over short-term grievances. But anyone who thinks, for example, that the grievances of Western Canada are short-term had better bone up on elementary facts.

Insufficient attention is given to the problem of a mature federal political body gaining political articulation by way of parliamentary institutions that reinforce rather than mitigate regional majoritarianism. There is no analysis of the importance of the reorganization of the Liberal party under Walter Gordon and Keith Davey into an organization incapable of anything but brokerage politics and short-term perspectives. No attention is given to Prime Minister Trudeau's belief in "rational management" of the economy nor to the inevitable and enormous growth that his ideological interventionism implied. There is no discussion of Trudeau's extraordinary obsession with his own version of the future of Quebec—an obsession that enabled him to choose his own successor, in another party, as prime minister. There is no focused analysis of the most important feature of Canadian politics, regional identities and conflicting regional interests that give bite to those identities. There is no discussion of the significance of political duplicity, as practiced by the Liberal Government in 1974 and 1975 over price and wage controls and in 1980 and 1981 over oil and gas taxes, for the continued health of liberal democracy in Canada.

By assuming that electorates can give various "mandates" to governments, the authors ignore the actual leadership given to the country during the 1970s. By relying so heavily on public opinion and voting data, the authors seem implicitly to have assumed the validity of a plebicitarian notion of Canadian democracy. In a federal, parliamentary regime led for the most part by a man such as Pierre Trudeau, it is not astonishing that the authors found an absent mandate. But it is not self-evident why they were looking for one.

BARRY COOPER

*University of Calgary*

**The State and Underdevelopment in Spanish America: The Political Roots of Dependency in Peru and Argentina.** By Douglas Friedman. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 236. $22.50.)

This is a neo-Marxist exercise in comparative political history that attempts to refute the notion that external factors (i.e., the evolution of the world capitalist economic system) determined the rise of primary export national economies in the postcolonial world. Using Peru and Argentina as case studies, Friedman argues that internal political factors, mainly the competition between emergent national elites for control of the nascent state, determined their eventual insertion into the capitalist world economy as dependent primary good exporters. Political victory allowed specific elites to assume control over the national state apparatus, which they then used to consolidate their positions of dominance in (and the very structure of) the national mode of production. In both cases, the social groups that eventually allied to gain control of the state by the late nineteenth century (when rapid economic growth confirmed the conditions of their position and role in the international economy) were the large land-holding interests and the export-oriented bourgeoisie.

In posing this counterfactual argument against the initial Leninist imperialist premise that sustained early dependency theory, Friedman restates and synthesizes the work of theorists such as Dos Santos, Gunder Frank, Cardoso and Faletto, Amin, and Wallerstein, as well as recent European Marxist polemics over the nature of the state, class conflict, and base-superstructure relationships. His reliance on Marx and more recent authors available in English such as Poulantzas, Miliband, Anderson, and Sweezy to develop this refutation is undermined by the fact that Friedman ignores the work of an intervening theorist (and *dependista* of sorts) who indelibly influenced the substance of modern Marxist exchanges, and who with several others shared the early quest for non-Leninist alternatives in Marxist theory and practice: Antonio Gramsci.

Even more problematic, although not surprising in light of what has been mentioned previously, is the fact that although it is excellently annotated and broad in scope, the book produces a sense of deja vu because it argues a point in a debate that has already been resolved. A number of Latin American scholars have long since incorporated the dynamics of internal political competition and national state formation into theories of dependent capitalist economic development. Several authors (Cotler, Kaplan, Laclau, Lechner, O'Donnell, Oszlak, Torres Rivas, and Zermeno, among others) have contributed significant studies that confirm the important role played by internal factors in the complementary processes of national state formation and economic and political development in Latin America.

A clue as to why Friedman would choose to argue an essentially moot (or at least conceded) point can be found in his bibliography and notes. All of his sources are in English. The implication is that he may be unfamiliar with non-English elements in Marxist theory (such as a 1982 edition of *Nova Americana* titled "Forme Storiche dello Stato" that is entirely dedicated to the subject of national state formation in Latin America). More worrisome is the fact that the possible lack of language training not only caused him to miss the original debate on this point argued in Spanish, but it also means that he was unable to document his case studies through use of primary sources such as archival material, government documents, and newspapers, or even non-English secondary sources. In other words, Friedman's entire argument is based on his interpretation of original and translated works by authors with the language ability to conduct primary data research in Spanish America.

That Friedman is able to cull this argument from a large and varied pool of English-language secondary sources is a tribute to his transposing skills and abilities as a rapporteur. Moreover, the book is one of the first English-language studies to discuss this subject at length. Unfortunately, that is more a reflection of the quality of the English-language literature than it is a comment on the book's merits. In addition, numerous grammatical and typographical errors detract from the overall presentation.

Thus, although it may be a helpful introduction to the subject for those lacking reading skills in Spanish or Portuguese, for the field in general this book constitutes another track on a well-worn road.

PAUL G. BUCHANAN

*Naval Postgraduate School*

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**The Brezhnev Politburo and the Decline of Detente.** By Harry Gelman. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 268. $29.95, cloth; $9.95, paper.)

During the 1970s, as Soviet-American relations entered and left the era of detente, Western views of the Brezhnev leadership and its attitudes toward the Western democracies tended to become increasingly polarized. According to Gelman, by the end of the decade those at one extreme of the spectrum of opinion saw the Soviet leaders as perpetually threatening, crude military aggressors in the manner of Hitler. At the other extreme, particularly in Europe, there were many who continued to believe that these same Soviet leaders showed an unrequited desire for permanent accommodation. Between these extremes, there were opposing tendencies. One current of thought assumed that events in the Third World during the latter part of the 1970s had alarming implications for the West, and that these events signaled a steady growth in the Soviet propensity to expand. An opposing trend of opinion argued that such events were part of a natural process, unpreventable and fundamentally unthreatening, through which the USSR was emerging as a great power on the world scene. One view stressed Soviet gains; the other, Soviet setbacks (p. 13).

Gelman's book, which has a place among the best analyses of Soviet politics and foreign policy during the 1970s—what many experts consider to be a unique period of detente and discord—is an important attempt to clarify the motivations, assumptions, and strategies underlying the foreign policy of the Brezhnev Politburo. Focusing upon the rise and fall of Soviet-American detente, according to Gelman:

> In the discussion that follows I argue that throughout the Brezhnev era a profound sense of vulnerability—over both the legitimacy of party rule and the stability of Soviet gains—powerfully reinforced the leadership's felt need to keep pressing for incremental gains within the scope of available opportunities and the limits of prudent risk. In consequence, I contend that from start to finish the consensus of the Brezhnev Politburo was highly pessimistic about the compatibility of Soviet interests with those of the United States. (p. 16)

*The Brezhnev Politburo and the Decline of Detente* is divided into five full chapters. The first seeks to reconstruct thematically the Soviet leadership's goals and fears in its worldwide interaction with the United States and to isolate those factors that drove the evolution of Politburo conduct during the Brezhnev era. The second examines, among other things, various structural characteristics of the internal Soviet political environment that helped determine the course of the political struggle within the Brezhnev Politburo. Chapter 3 traces certain events in that internal struggle and their subsequent effects upon the attitudes the Brezhnev leadership brought to detente. The fourth discusses the evolution of the Soviet detente relationship with the United States within the historical competitive context of external Politburo behavior from which it first emerged in the 1970s, and into which it eventually returned. Finally, Chapter 5 considers the prospects for Soviet-American relations in the post-Brezhnev era against the background of the "asymmetrical vulnerabilities" of Soviet and American societies (p. 17).

In reference to the international sphere, Gelman thoroughly discusses the rise and fall of the detente relationship and covers many of the factors that influenced its development and decline: the China factor, technology transfer, the SALT process, Soviet calculations in Africa and Southeast Asia, Soviet interaction with Egypt, third-world politics and Soviet opportunities, Soviet interaction with American pluralism, and the invasion of Afghanistan.

*The Brezhnev Politburo and the Decline of Detente* is a solid work that will be of interest and value to students of Soviet studies, international affairs, and political science, as well as anyone interested in Soviet-American relations during the period of detente and the prospects for East-West relations in the 1980s.

JOHN M. CARFORA

*Dartmouth College*

**The Dynamics of the One-Party State in Zambia.** By Cherry Gertzel, Carolyn Baylies, and Morris Szeftel. Edited by Cherry Gertzel. (Manchester: Manchester University Press, 1984. Pp. xi + 262. $35.00.)

A truly collaborative effort among three scholars who have spent a number of years in Zambia, this book continues the intellectual tradition established in the two books edited by William Tordoff, *Politics in Zambia* and *Administration in Zambia,* which were published in Britain by Manchester University Press in 1974 and 1980. This tradition, which can be designated the Manchester school of Zambian political studies parallel to the earlier Manchester school of Zambian anthropological-sociological studies, is both the dominant school in terms of number of publications and the most sophisticated one yet to appear. Furthermore, it is constantly evolving, and the book under review makes significant contributions to that evolution.

The central concern of this book, like that of earlier books in the same tradition, is on the influence of various forms of social relations on changes in political institutions and political conflict. The major innovations in the content of that central concern that are made in this book are a more explicit treatment of class and greater emphasis on central-provincial relations. More concretely, the emphasis of the book empirically is on the interaction of social relations, political conflict, and political institutions in the 1973 and (to a lesser extent) 1978 one-party elections. Candidates, campaigns, voter turnout, central party control, and social and political factors determining results are discussed in detail. The methodology employed is the combination of electoral data and theoretically informed narrative description that is found in all of the Manchester school books.

Having read a number of articles and manuscripts by Baylies and Szeftel, I was surprised that class analysis did not occupy a more central position in the work under review. The probable reason for this is the difference in perspective among the authors that is mentioned in the Preface. One chapter is devoted to "the rise to political prominence of the Zambian business class," but this chapter focuses primarily on the electoral success of candidates with business backgrounds in 1973 and 1978 and does not reveal the sophisticated theory and methodology that Baylies and Szeftel employ elsewhere to determine the nature and influence of this class. Class forces as determinants of political conflict and institutional change are discussed in other chapters, more so than in previous Manchester school books, but remain a secondary theme in most of the book under review.

Almost half of the book is devoted to three case studies of central-provincial relations: Copperbelt, Luapula, and Western provinces. The first of these is shown to have lost much of both its political preeminence and its enthusiastic support for the governing party, UNIP. These trends are accounted for by the economic difficulties of the copper industry, the conflict between the government and the labor movement, the prominence of a partially ethnically based opposition in the province immediately before the introduction of the one-party state, and the substantial weakening of party organization. Although they have had very different political histories, the Luapula and Western provinces are now quite similar as two of the three most underdeveloped rural provinces. Although support for UNIP has also declined markedly in Luapula and has been weak in the Western province since shortly after independence, the authors do not feel that this rural alienation is as much of a problem for the regime as urban alienation on the Copperbelt because of the greater economic importance of the latter area and the presence there of alternative objects of loyalty—the trade unions.

Although greater theoretical integration and more precise measurement of variables in the study of change in Zambia are to be hoped for in the future, this book makes an important contribution and should be read by all students of the Zambian one-party state.

JAMES R. SCARRITT

*University of Colorado, Boulder*

**Quebec, the Challenge of Independence.** By Anne Griffin. (Cranbury, N.J.: Farleigh Dickinson University Press, 1984. Pp. 220. $27.50.)

The existence of a large and important French-speaking province in the heart of a culturally different Canada offers an attractive subject for comparative scholarship; there is now a growing body of nonpolemical literature on Quebec's political role and—more particularly—on separatism.

This volume by Anne Griffin represents an unusual approach to the topic because it is primarily concerned with the personal responses of Quebecers who support the independence of Quebec. The research question concerns the reasons selected individuals want independence and the way their lives have changed as a result of their political commitment. The main vehicle for participation for most of them is the Parti Quebecois, which was voted into power by the people of Quebec on November 15, 1976 and pursued its quest for autonomy from English-speaking Canada through the next four years. Defeat of the referendum on "sovereignty-association" in May, 1980 was hailed throughout Canada as a statement that Quebecers had firmly rejected separatism but, as Griffin and others point out, it really quieted little of the sentiment for an independent identity. Levesque continued in power and would represent his province at the constitutional talks on patriation, although he would be in the awkward position of attempting to negotiate a constitutional arrangement acceptable both to the voters of Quebec and the Parti Quebecois.

Griffin carefully documents the uneasy relationship between French and English-speaking Canadians over four centuries of coexistence emphasizing "the politics of survival" and then examines 11 ways of looking at separatism. Intensive interviews and conversations were conducted with 23 individuals, 14 of whom believed in separatism. Eleven of these encounters are described in the case reports that constitute the second part of the book.

Reminiscent of Robert Lane's classic analysis of the political ideologies of "common men," which showed that acts of participation (and nonparticipation) can satisfy psychological needs, Griffin explores the way in which separatism helps secure the mental well-being of French Canadians. Although she builds a rather elaborate explanation on a slim empirical base, the result is nevertheless valuable.

This book is a useful addition to the English-language literature on Quebec separatism.

KEITH HENDERSON

*State University College, Buffalo*

**The Policy Process in the Modern Capitalist State.** By Christopher Ham and Michael Hill. (New York: St. Martin's Press, 1984. Pp. xiii + 210. $29.95.)

Hugh Heclo's 1972 review article on "Policy Analysis" (*British Journal of Political Science, 2,* 83-108) deserves much of the credit for a booming interest in areas once considered little more than a subset of public administration. Comparative public policy, policy analysis, and program evaluation have since become important research fields in their own right; hence, a state of the art book that also incorporates future directions would be most welcome after a decade of extensive empirical research.

Rather than servicing this need for synthesis and guidance, Ham and Hill's book (which is the result of their involvement with a Master's level public policy studies program at the University of Bristol) amounts to an old-style, albeit fairly well-written, review of the literature. It traces the evolution of organizational sociology approaches by summarily treating a battery of "pioneer works" dating back to Marx and Weber. However, the work tends to neglect the very areas in which the theoretical frontier has expanded significantly over the last five years, for example, in relation to design science and studies of environmentally induced complexity. The authors contend that "there is a need now for good empirical studies to link the macro and micro levels of explanation" (p. 189) regarding the determinants and consequences of government action; but aside from passing references to "radical organizational theory," they offer few insights as to what form these linkage studies ought to assume. The examples concentrate on intraorganizational dynamics, undermining what would appear to be the main thesis that both the analysis of policy and analysis for policy are constrained by existing external socioeconomic relationships.

The introductory chapter, "Policy and Policy Analysis," sketches distinctions made by earlier theorists with respect to policy content, policy process, policy outputs, evaluation, information gathering, process advocacy, and policy advocacy. We are told that the study of process is the key to understanding the evolutionary character of policy, and that the authors would prefer to see politics prevailing over markets in public decision making. Yet the rest of the text concentrates on the more or less static, self-reinforcing aspects of bureaucratic behavior. Addressing the "Role of the State," chapter 2 discusses Ralph Miliband, James O'Conner, pluralist theory, elite theory, non-decisions a la Peter Bachrach and Morton S. Baratz, and neocorporatism. Chapter 3, "Bureaucracy and the State," makes some effort to link Weberian and Marxian perspectives to elitist and corporatist theory, but it does not explain what a focus on specific actors in the policy process could contribute to our understanding of the transformation of the modern (postindustrial as opposed to the Keynesian?) capitalist state as a whole.

In fact, chapters 4 through 9—"Power and Decision Making," "Rationality and Decision Making," "Implementation Theory," "Organization and Policy Process Studies," "Bureaucrats and the Policy Process," and "Discretion"—lose sight of specifically capitalist constraints altogether: Ham and Hill do not carry out their self-imposed mandate to "give due consideration to the social, political and economic contexts within which problems are tackled" (p. 174). Questions of public finance management are never raised. The lack of attention to specific problems of interinstitutional competition for scarce resources tempts me to paraphrase a once-popular T.V. commercial, "Where's the state?" The cross-cutting influences on the actions of bureaucrats (e.g., conflicts between personal objectives versus organizational goals) described are not limited to a particular ideological system.

The results of the concluding chapter, "Linking Levels of Analysis," are equally disappointing. At best, the authors note their agreement or disagreement with 16 direct quotes and 23 references to other theorists hastily covered in the last 16 pages. Presumably the new directions for policy analysis rest with the development of "Radical Organization Theory," whereby the most impressive examples of RTO-applied (Charles Perrow, Mary Zey-Ferrell, Michael Aiken, Frank Fischer and Carmen Sirianni) receive no mention in the text. A closing endorsement of other writers' prescriptions (p. 188) is not to be equated with an original contribution: In this case the "art's" progress has been quite understated.

JOYCE MARIE MUSHABEN

*University of Missouri-St. Louis*

**Decolonization in Britain and France: The Domestic Consequences of International Relations.** By Miles Kahler. (Princeton, N.J.: Princeton University Press, 1984. Pp. xiv + 426. $40.00, cloth; $9.95, paper.)

This book represents a fine combination of scholarship: a meticulous attention to historical sources, a concern for detail and nuance, and an interest in theoretical propositions concerning reciprocal relationships between international events and domestic policies. Its major theme is that Britain and France were not merely actors in their imperial roles, but that their foreign and domestic policies were heavily influenced by the empires they controlled. There is an emphasis on the linkage of a number of variables—international pressures, settler activities, economic forces, and the contagion of developments in other countries—that all help to explain the evolution of party ideologies and government policy choices.

In pursuing his analysis, Kahler clears the field of inadequate or mistaken approaches—these are sometimes straw men he has himself erected—such as Marxism, dependency theory, and the failure of scholars to consider external forces when dealing with domestic changes. Thus, he refers to Machiavelli to bolster his contention that "apart from the literature of imperialism and dependency, societies have for the most part been studied in isolation from . . . each other" (p. 14). Kahler pays considerable attention to the "organizational dimension of ideology" which, in his view, has been consistently ignored (p. 61) in favor of a stress exclusively on the belief system of individuals. Yet his own approach suggests a certain ambivalence. On the one hand, he is influenced by structuralism in emphasizing that policies emerge in a holistic fashion, that is, are responses to preexistent contexts; on the other hand, he has large sections of discussion—and they are among the most interesting parts of the book—of the individual choices made by prominent politicians. His conceptual ambivalence leads Kahler to emphasize both the relative autonomy of the state with regard to its own groups and elites (p. 36) and its lack of autonomy with regard to the external world.

Kahler makes the important point that it was easier for the empire to "penetrate" the French polity than the British one, because the French Right was organizationally more unstable than the British and because its "imperialist ideology" was ambiguous, although later in the book he concedes (but discusses all too briefly) the fact that imperialism was also a component of the Jacobin Left. In dealing with party cohesion, he stresses the notion that the "threat of exit," that is, of adherents leaving one party for another, is greatest in "a multiparty system in which the party face[s] a number of close competitors" (p. 66); this threat presumably explains the relative loyalty of supporters of the Labour and Tory parties, a loyalty that in turn gave these parties a more or less free hand. But the argument can be reversed: In British and other two-party systems, parties tend to resemble each other so that "exit" is not too difficult ideologically, whereas the ideologism that is associated with a multiparty system (as in the Fourth Republic) may be important enough to make switches difficult. Kahler may be on more solid ground in his argument that decolonization was easier for the British than for the French when he cites the fact that (except for Southern Rhodesia) there was much less settler resistance in the British colonies than in the French ones.

There is occasional murkiness of style and logic, for example, (p. 9): "Analysis of the social groups that are particularly sensitive to nationalist appeals and external threat correctly predicts the trials of France at the hands of rebellion within the state." More substantively, there are certain biases that not all readers may share. Thus, in contending that the British political system, in particular the party system, has been more resilient to decolonization than that of France (during the Fourth Republic), Kahler is much kinder to Britain than to France: He is excessively harsh on the SFIO and especially on Guy Mollet, who comes across as an unprincipled opportunist for his alleged failure to utilize his ability to control dissent (p. 225) in behalf of anticolonialist ends.

Conversely, Kahler is rather tolerant of the Gaullists, as when he asserts (p. 77) that Gaullism "did not employ nationalism in its political strategy; it *was* nationalism." This point might be argued, in view of de Gaulle's implied promise—"je vous ai [com]pris . . ."—to the *colons* in North Africa; in fact, Kahler (p. 80) attributes the RPF's opposition to decolonization to its anticommunism. Similarly, he insists (p. 101) that the Gaullists' embrace of *Algérie française* was not a tactic to bring down the Fourth Republic, but later (p. 102) he refers to "the Gaullist strategy . . . to encourage plans for military invasion of the metropolis . . . in order to cause a collapse of resistance to de Gaulle's return, and, at the same time, to restrain such direct action to ensure that the transfer of power had a semblance of legality." The book contains other interesting details about the evolution of Gaullism from 1958 to 1962 and the efforts at making the movement politically *salonfähig,* to the point where Gaullism as an ideology became "full decolonized" (p. 110).

The treatment of the ad hoc political responses of, and the ideological impact on, the Conservative and Labour parties is detailed in its chronological dimensions, but there are some omissions. Kahler makes only passing mention of Ireland and Palestine; while acknowledging the policies regarding these areas as failures (p. 386), he argues that Britain turned even these into successes by "externalizing" them. There is no discussion of embarrassing instances of imperialist behavior within the Labour party—from the "bleeding heart" paternalism of certain Fabian intellectuals at the turn of the century to the romantic Arabism (and not so romantic anti-Semitism) of Ernest Bevin and other postwar Socialists. (Kahler does mention Bevin's "neo-mercantilism.") There is only a cursory treatment of the impact of "colored" settlers in Britain on party ideologies and programs or on the pattern of race relations, and there is no discussion of Britain's economic decline—a phenomenon that might possibly be considered a "systemic" failure to adjust to the loss of colonies. Finally, it is curious that in a book devoted to external influences there is no mention of Anthony Eden's resignation as prime minister, let alone of any possible American influence on that event.

But these are minor shortcomings that do not detract from the quality of this comprehensive and engrossing work, which is a major contribution to an understanding of the impact of decolonization of the colonizers.

WILLIAM SAFRAN

*University of Colorado, Boulder*

**The Decade of Energy Policy: Policy Analysis in Oil-Importing Countries.** By Paul Kemezis and Ernest J. Wilson, III. (New York: Praeger Publishers, 1984. Pp. xvi + 271. $27.95.)

The oil production cutbacks and embargo of 1973 opened a decade of rapid and unpredictable fluctuations in world energy markets. Kemezis and Wilson describe the consequences as an "energy policy explosion." The governments of the more than 100 oil importing countries had shared objectives: "to obtain secure energy supplies from abroad at reasonable prices in the short term while promoting increased domestic production, efficient use and fair allocation of energy in the long term" (p. 29). Despite the similarity of the objectives, policies aimed at achieving them differed greatly across countries. The goal of *The Decade of Energy Policy* is to describe and explain the variety in policy outcomes.

One example of this variability concerns short-run policies aimed at securing supplies in the short term. Some governments merely expanded their information services; others negotiated directly with producer governments; some took over the purchasing of oil completely. Factors including "relations with the major companies, world political posture, and attitudes toward state-controlled companies" (p. 80) are used to compare the policies of France, Italy, Japan, West Germany, Denmark, Brazil, Korea, and Kenya, among others (pp. 96-102). A deeper theme running through this discussion is that political means emerged to replace economic arrangements rendered unreliable by the volatile world market.

In the latter part of the book, the authors review the array of policies governments used to enhance domestic energy supplies (pp. 118-177) and reshape or restrain demand (pp. 178-214). The closing discussion addresses directly the politics and administration of energy policy. The authors show how in many countries energy policy both became a national ideological and political issue and was itself shaped by other such issues. The lines around which group conflict over energy policy was most likely to emerge included producer-consumer differences, competition between proponents of different energy sources, supply-side versus demand-side philosophies, the issue of whether to shift policy costs or benefits forward to consumers or back to producers. In effect, the "energy policy explosion" reflected in part the politicization and bureaucratization of what had been an essentially technocratic decision-making process (pp. 218-219).

A particular strength of this book is that it is rich in facts, insights, and interpretations concerning the energy policies of oil importing countries during the decade from 1973 to 1983. Clearly drawing on a wealth of first-hand information, Kemezis and Wilson make good their promise to detail the differences among national policy responses to the rapidly shifting energy market in its many economic and political aspects. Also, the book is well integrated. The authors regularly link specific discussions to the overarching theme of increased government influence in energy.

At the same time, there are certain things the reader will not find. First, the authors choose to review energy policy one issue at a time, rather than one country at a time. The reader interested in a coherent overview of the oil policy process in a single country will have to dig for it. Another point has to do with inattention to details of language; obvious examples appear on pages 40 ("most unique"), 93 ("principle area"), 106 (sentence fragment), 148 (the "affect" of taxes), and 179 ("the . . . impacts . . . is why").

Notably absent is any direct reference to the theoretical literature of organizational theory and policy analysis. Every one of the more than 120 bibliographical listings deals with energy policy. The wealth of fact and analysis in this book asks for integration into a broader conceptual framework, but none is suggested. One candidate might be the typology Gary Wamsley and Mayer Zald offer in *The Political Economy of Public Organizations* (Indiana University Press, 1976). Kemezis and Wilson offer numerous insights into the interconnections of internal and external political and economic factors affecting policy outcomes. Reference to a broader synthesizing framework could extend the appeal of this book beyond audiences concerned mainly with the energy policy process itself.

In sum, the reader seeking to be sensitized to and informed about the interplay of factors shaping energy policy in a range of oil importing countries after 1973 will find this book insightful and helpful.

JOHN HEILMAN

*Auburn University*

**Wagnerism in European Culture and Politics.** Edited by David C. Large and William Weber. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 361. $34.50, cloth; $14.95, paper.)

The figure of Richard Wagner remains on the periphery of this fine book that has as its real subject not Wagner, but Wagner*ism*—the cultural, intellectual, and political movements inspired by Wagner. Loosely based on his music dramas and his pronouncements as self-appointed cultural and social messiah, a significant if diffused Wagnerism pervaded European intellectual life in the last third of the nineteenth century and the first two decades of the twentieth. As the editors point out in their introduction, the individuals and groups attracted to Wagnerism "shared deep reservations about aspects of their society and culture and were looking for a vital new alternative" (p. 16). Disillusioned with positivist thinking, political liberalism, religious orthodoxy, and conventional sexuality, the Wagnerians saw this alternative embodied in one or another aspect of Wagner's legacy.

This book presents the most thorough survey of Wagnerism available in English. After an opening essay by William Weber on "Wagner, Wagnerism, and Musical Idealism," the core of this survey is a series of essays that explore the personalities, ideas, and institutions of Wagnerism in six nations. David C. Large writes on "Wagner's Bayreuth Disciples," Gerald D. Turbow on "Art and Politics: Wagnerism in France," Marion S. Miller on "Wagnerism, Wagnerians, and Italian Identity," Bernice Glatzer Rosenthal on "Wagner and Wagnerian Ideas in Russia," and Anne Dzamba Sessa on "At Wagner's Shrine: British and American Wagnerians." The image of Wagnerism that emerges from these essays is much more complex than the currently accepted association between Wagnerism and Nazism. David C. Large documents the undeniable connection between Wagner's Bayreuth circle and Nazism, but the other essays suggest a more varied political character in other countries, including a belated infatuation with Wagnerism by the Bolsheviks through the late twenties.

Although this book argues for the appreciation of Wagnerism in any comprehensive account of European ideas in this period, it never overestimates its subject. Wagnerism is depicted as a spent force by the end of the twenties, and its disastrous resurrection by the Nazis will probably preclude any future revivals. Moreover, even in its heyday, Wagnerism was no more than a thread in the fabric of European thought and culture. Wagnerism never supplanted liberalism, Darwinism, nationalism, or socialism, but rather mingled with these ideologies to produce intriguing nuances and combinations.

This book avoids the usual pitfalls of anthologies. *Wagnerism in European Culture and Politics* is not an uneven collection of vaguely related essays, but a well-conceived and well-executed piece of cooperative scholarship. The writing is clear, and the scholarship is scrupulous throughout. The editors' Introduction and Conclusion are cogent and helpful. And the ample notes are a godsend for future work on this and related subjects. I highly recommend this book.

JOHN BOKINA

*Pan American University*

**Ideologi och Strategi: Svensk politik under 100 ar.** By Leif Lewin. (Stockholm: Norstedts, 1984. Pp. 413. SEK 136.00.)

For several years Uppsala scholars have made studies of Swedish politics "as rational conduct," and this is the final, concluding work. It is an ambitious achievement. It examines eight momentous policy decisions: protectionism (1888), extension of the franchise (1907), parliamentarism's acceptance (1917), the "horse-trade" behind the full employment program (1933), the acceptance of welfare state, mixed-economy planning (1948), supplentary pensions (1958), nuclear power (1976-1980), and wage-earner funds for purchase of corporate stocks (1983).

The choice of cases, one must suppose, did not hinge solely on the wish to test a hypothesis. Lewin chooses matters of historic importance and writes a dramatic account of each to produce a book attractive to the general reader as well as the specialist.

Lewin's choice of analytical tool is game theory, which he applies to each case. He lays out matrices to show choices and payoffs open to the political actors. He strives to show that the choices actually made were what one might have expected of committed, calculating human beings. Thus were the political actors rational and the outcomes explicable.

The explanations have the character of post hoc rationalizations, which means they cannot be tested. Lewin however claims no more for game theory than that it provides an "ideal type" by which to take stock of politics in the real world. The great virtue of this approach is that it advances the cause of seeing politics as politicians themselves do.

Lewin denies his analysis is normative, and indeed it is true that he takes a detached, neutral view of the issues. Nevertheless, he has an obvious liking for the rules of the political game that have evolved in his country. A trace of cultural bias is also evident in the special meaning of "ideologi" in the book's title; "ideologi" here means principles and convictions, not creeds and dogmas. Accordingly, a flexible, pragmatic party that adopts a bargaining "strategy" to fulfill half of its "ideology" (principles) exhibits "rationality" in a way in which an inflexible, uncompromising party does not. This analysis fits the Swedish case, but not necessarily others where cultural traditions differ.

Lewin draws extensively on recent game theory innovations such as Norwegian scholar Jon Elster's refinement of the prisoner's dilemma into a solidarity game. He applies this to show how opposing interests in the late 1940s found it mutually advantageous to cooperate in laying the basis for

Sweden's mixed, semiplanned economy. The account is elegant, and it causes one to think of Robert Axelrod's speculations in this journal on the evolution of cooperation in human society in general.

This book is both simple and profound in the best sense of those terms. It appears naive in that its game theory element is simple and mathematically unembellished. But it is profound in that it treats political choices seriously. In a masterful concluding chapter Lewin discusses the chimeral nature of majority rule, which all revere but which does not work unaided. The credit for getting good results out of majoritarian decision systems is what Lewin (invoking Machiavelli, Max Weber, and Herbert Tingsten) is prepared to give to foxy politicians.

BERTIL L. HANSON

*Oklahoma State University*

**Civil Religion in Israel: Traditional Judaism and Political Culture in the Jewish State.** By Charles S. Liebman and Eliezer Don-Yehiya. (Berkeley: University of California Press, 1983. Pp. x + 305. $24.95.)

This is a book about the divergent elements of modern Jewish nationalism-Zionism. Nationalism, as George Orwell reminds us, can turn into many things:

> When nationalism first became a religion, the English looked at the map, and, noticing that their island lay very high in the Northern Hemisphere, evolved the pleasing theory that the further north you live the more virtuous you become. (*The Road to Wigan Pier,* Penguin Books, 1962, p. 100)

Has Zionism become a religion? Liebman and Don-Yehiya believe that in the past, the following "civil religions" dominated: Zionist-Socialism, Statism—and over the last 30 years—a "new civil religion." These three "religions" comprise the main subdivisions of the book, which focus on the impact of traditional Judaism, and on the values, symbol systems, myths, rituals, and heroes in each.

This analysis of the intricate mosaic of Zionism provides, for instance, a look at the conflicting values within Zionist-Socialism or the changing roles of Judaism in Israeli political culture. The authors also observe that recent developments in Israel affirm:

> . . . the notion of public rather than private observance. . . . Whereas Judaism places man's obligations to God at the center of its value system . . . the new civil religion places the individual's obligations to the nation at the center.

This is a valuable *political* insight into the so-called "revival of religion" in Israel after 1967.

But why call it a "civil religion"? Civil religion is like traditional religion, we are told, except that at its core stands a corporate entity rather than a transcendent power. Although God has been removed, and "there can be societies without a civil religion" (p. 11), the authors suggest that Israel is a "visionary" rather than a "service state," and therefore "requires a civil religion" (p. 216). This observation could have been sharpened by stating that the history of Israel shows that Israel needs an "overarching, integrative symbol system" (p. 11), and that after experimenting with different symbol systems, the *current* emphasis is on shared Jewishness and Jewish continuity.

Another problem is whether one can explain the development of Israel's political culture in terms of "religious" encounters alone. Both conflicting and identical elements exist in each "civil religion"—without dividing them into artificial stages. Zionist-Socialism has been, from its inception, both universalistic and nationalistic (see, for example, the discussion of May Day celebration); secular and traditional (in the cultural sense); and even "statist" before the state was established, having strong affinities with the disguised étatist sentiments of the New Hebrews movement. Statism adopted elements of extreme secularism, but also embraced traditional references and a notion that Israel is the expression of the national Jewish spirit. The new "civil religion" combines old and new elements but, unlike its predecessors, "lacks a coherent ideological formulation" (p. 135). In short, the familiar story of continual groping; the uncertain evolution of Jewish nationalism in the modern era. My argument, therefore, is not just with the artificial stages, but primarily with breaking down the process of Israel's political-cultural evolution into different "civil religions."

Based on the excellent material presented in this book, the central problem of modern Jewish nationalism appears to be the "loyalty blend": how much of the Judaism, Jewishness, and "the Jewish people" concept can be blended with the secular, modern, democratic nation-state concept. In the most important secular movement of the pre-State era, Zionist-Socialism, a combination of universal and nationalistic elements, prevailed. Revisionist Zionists presented a more simplified and romantic notion of nationalism—a combination of tribal Jewishness and *etatism.* The ultra-orthodox Jews completely rejected modern nationalism, while the more moderately religious have tried to cope with it in different ways (p. 185). After the Jewish State was established, crude state-nationalism prevailed for a while as was to be expected, but a complex "loyalty blend" soon reemerged. Thus, the new "civil religion" is an attempt to reconcile the identities of "Israeli" versus "Jew" by relying more on tradition and on Jewishness than in the past.

But have we resolved the schism? Is there a clear-cut distinction between statism and the current situation? I find this new "civil religion" both strongly statist and nonstatist. Some religious Jews, for example, have become more extreme in their rejection of the State of Israel, while others have developed messianic visions colored by a fanatic chauvinism that is political, that is, tied to the state as the vehicle of redemption.

The authors assert that Israel, like other Western societies, has experienced "a decline in nationalist sentiments" (p. 136). Modern Jewish nationalism is hardly 100 years old, however, and the searching process continues. Recent trends, including the growing importance of traditional Jewish symbols in Israel, are as precarious as the previous ones. After all, God has reentered, but in name only (p. 136), and one may even say as a national hero. The emphasis on Israel as a Jewish state has been accompanied by a crisis within the religious camp itself. Hence, we may yet witness a neo-civil creed, a rediscovery of some of the old, universal elements, a return from chauvinism—religious or secular—to "normal nationalism."

"We are not happy with the new civil religion," the authors conclude (p. 237), and I share their general sentiment. I think, however, that the case could have been argued differently. Nevertheless, this is an important book because it raises some of the most crucial questions about the political culture in Israel.

ITZHAK GALNOOR

*Hebrew University of Jerusalem*

**Democracies: Patterns of Majoritarian and Consensus Government in Twenty-One Countries.** By Arend Lijphart. (New Haven, Conn.: Yale University Press, 1984. Pp. xv + 229. $22.50, cloth; $6.95, paper.)

Lijphart's study focuses on 21 democracies which, using a variety of formal and informal institutions and practices to turn citizen preferences into policy, have continually existed for the last 30 to 35 years. The primary objective is to present the major institutional characteristics of these regimes in such a way that one is able to systematically compare them, drawing nine criteria for comparison from two abstract models of democracy: the majoritarian model and the consensus model. On the basis of this systematic study, Lijphart categorizes these democracies into groups of majoritarian and consensus regimes and explains why democracies belonging in one group use institutions and practices which, according to the two abstract models, seem to be theoretically inappropriate.

Although Lijphart achieves this objective, using tables to summarize his findings, his attempt to analyze and compare patterns of government of 21 democracies in 244 pages leads to some difficulties, most of which can be attributed to a lack of detail. For example, because the objective is to analyze the patterns and regularities of the 21 regimes, one might expect that each regime would be given equal treatment in the discussion accompanying the tables. Unfortunately, such is not the case. Relatively little attention is given to some of the democracies, especially Japan and Israel.

A more important problem is found in Lijphart's selection of the 21 cases of successful democracy. Although he recognizes that "France is a somewhat doubtful case because its regular democratic processes were briefly suspended during the transition from" (p. 39) the Fourth to the Fifth Republic, France is still included. In fact, Lijphart includes both republics as separate examples of equally successful democracy even though neither strictly fit his criterion for inclusion in the study. It is not clear why two other democracies that he recognizes as marginal cases, India and Costa Rica, could not have been included in the same way.

The inclusion of France points to the main difficulty in this study: the handling of another theme that is advertised on the back cover as the major theme of the book, "There is more than one way to run a successful democracy." The problem lies in defining successful democracy as any democracy with a longevity of approximately 30 to 35 years. Although this definition provides a clear set of cases for evaluation (with the exception of France), the argument is circular. Lijphart has chosen to analyze democracies which, by definition, are successful instead of demonstrating how the different institutions of majoritarian and consensus democracies contribute to the longevity of a regime. Moreover, as the inclusion of the French Fourth Republic indicates, his definition of success does not seem to be closely related to regime stability. There are numerous examples of failed democracies (some of which are mentioned in the study) which existed long enough to fit Lijphart's criteria of success.

An understanding of why these 21 democracies can be considered to be truly successful requires an analysis of why a large number of democracies have failed to survive, or have tended to be unstable. An important question that could be addressed by comparing these 21 regimes to failed democracies is, Which tend to be more stable: majoritarian or consensus democracies? If both models appear to be equally stable, why have these democracies survived and others failed? If both are equally unstable, what accounts for the longevity of the 21 democracies analyzed in this study? Lijphart seems to point to some answers early in his study, but those answers have little to do with the institutional patterns that are the focus of his book.

DAVID R. HERRON

*Severna Park, Maryland*

**Environmentalists: Vanguard for a New Society.** By Lester W. Milbrath. (Albany: State University of New York Press, 1984. Pp. vii + 180. $29.50, cloth; $9.95, paper.)

Milbrath's book is an analysis of a three-nation survey (the United States, England, and Germany) conducted in 1980 and 1982 which focused on the general knowledge, concern, and commitment to environmental issues by elites (political, business, labor, and media leaders) and the general public. Milbrath's fundamental thesis, based upon these surveys and other public opinion polls, is that a silent revolution is taking place in these three nations, with the emergence of an environmental vanguard. Moreover, Milbrath envisions a new, dominant belief structure, labeling it the New Environmental Paradigm (NEP) that in his view is gradually replacing the Dominant Social Paradigm (DSP). Furthermore, Milbrath argues that the most significant dimension of politics is now the constellation of environmental attitudes of mass publics rather than the traditional left-right continuum.

Milbrath establishes a typology of environmentalists (ranging from "The Rearguard" through the "Undecide Middle" to "The Vanguard") and their environmental sympathizers. He argues persuasively that occupational status rather than social class best explains the differences in environmental concern of these various groups. Individuals employed in the service industries in the economies have adopted the NEP while those employed in the "production sector" remain committed to the DSP.

Contrary to most recently published works, Milbrath remains guardedly optimistic about the future of the environmental movement. He cites as evidence past successes such as the population writers, the impact of Rachel Carson's *Silent Spring*, the environmental legislation of the 1970s, the recent electoral success of the Greens in Germany, and the continued support for environmentally sound policies in his survey data. He recognizes that the political mobilization of environmental groups is not sufficient to change attitudes, but sees future ecological "crises" and acute resource shortages, especially in energy, as providing the catalyst to the adoption of the NEP. No doubt, Milbrath would point to the disaster in Bhopal, India in December, 1984 as a classic example of such an environmental catastrophy that will further enhance the coming of the NEP.

Milbrath has produced a book sure to be cited by environmentalists. This book is one of the few survey research studies published that provides the reader with the extensive detail regarding sampling procedures, the complete survey instruments employed, and the elaborate breakdown of the tabulated data so necessary for an accurate assessment of the reported research. In spite of the soundness of Milbrath's methods, many of his inferences about the future success of the environmental movement rest more on his hopes and aspirations for it than on the empirical data. The fragility of the data is of special concern in light of the fact the surveys were completed only in the two years 1980 and 1982. Because I share many of Milbrath's values, I hope he is correct; however, I should like to see his surveys replicated in the near future to corroborate his findings and give more credence to his interpretation.

DAVID D. DABELKO

*Ohio University*

**The Politics of Women and Work in the Soviet Union and the United States: Alternative Work Schedules and Sex Discrimination.** By Joel C. Moses. (Berkeley: Institute of International Studies, University of California, 1983. Pp. xi + 181. $9.50, paper.)

Alternative work schedules have become an important policy option for organizations since the 1970s. Joel C. Moses's book analyzes the development and initial implementation of alternative work schedules in the Soviet Union and the United States. Within the context of comparative public policy, Moses seeks to compare the political and economic factors influencing the development of public policy concerning alternative work schedules. This important comparison explores issue salience, interest groups, political factors, and opposition for two countries which, although supposedly very different in ideological philosophy and political processes, developed similar public policies with respect to alternative work scheduling.

According to Moses, alternative work schedules

(AWS) encompass any modification of labor policy requiring that all regular employees adhere to the same prescribed times and number of hours worked daily or weekly. Common types of AWS include staggered work hours, flexible work hours, compressed work weeks, permanent part-time employment, and work-sharing (p. 7). Moses indicates that there are basically three reasons why organizations (including governments, of course) implement alternative work schedules: increasing efficiency of current workers, surplus in the labor market, and the concerns of women. In addition, Moses mentions four trends that have influenced employment policies in recent years: inflation, increasing percentage of adults in the labor force, policies to conserve resources, and automation. According to Moses, these factors differ in relative importance in the Soviet Union and the United States.

By comparing U.S. federal policy with USSR all-union policy, Moses concludes that policy substance is converging, but that the political processes for policy determination differ significantly. In research based on documents and interviews in both the United States and the Soviet Union, Moses concludes that, while in both countries the debate was relatively nonpolitical and low in profile, policy development in the United States has advanced due to the pressure of women's organizations, whereas policy development in the Soviet Union has advanced due to economic necessity. This conclusion seems suspect for methodological reasons. It is far easier to obtain information about interest group activity from the United States; whereas in the Soviet Union interest group information is lacking, and it is easy to fall back on a residual category of "economic causes."

Moses's technical, yet unconvincing argument rests on a rather free interpretation of demographic data, government reports, and interviews. Although the notes are extensive, the book lacks a bibliography. Although titled *The Politics of Women and Work,* the book might have been titled *The Economics of Employment, with Occasional Reference to Women.* With the changing roles of women, the uncertainty of the impact of employment policies on the well-being of women, and the increasing interdependence and communication among world-wide policymakers, such a book is crucial to our understanding of women and work. Unfortunately, this book presents women more as passive receivers of policy than as active participants in the policymaking process. Even the description of the process in the United States, which is twice as long as the section on the Soviet Union, emphasizes contextual economic considerations more than the roles and interests of women. Moses's book features the U.S.A. at the expense of the USSR and the economic at the expense of the political. It is an interesting venture in the area of comparative public policy, but it does an injustice to sound methodology and to women and their concerns.

JEANNE-MARIE COL

*Sangamon State University*

**Bureaucracy, Education, and Monopoly: Civil Service Reforms in Prussia and England.** By Hans-Eberhard Mueller. (Berkeley: University of California Press, 1984. Pp. xi + 301. $37.50.)

**The Making of a State: Württemberg, 1593-1793.** By James Allen Vann. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 321. $29.50.)

The historical growth of government is usually taken to be the inevitable outcome of impersonal forces. These two books challenge those orthodox assumptions of inevitability and impersonality. Governments did grow, and they did change as they grew, but that growth and change required the efforts of many individuals, and their efforts were not always successful.

Vann specifically warns against a linear reading of the history of the state. Because of the time span his book, *The Making of a State,* covers, he has ample opportunity to demonstrate the false starts, the failures, the drifting, and the regressions that occurred. Vann's study of the making of the state of Württemberg emphasizes the development of a professional bureaucracy. Like Mueller, Vann is frequently at odds with conventional interpretations of his subject. On bureaucracy, he argues that the Württemberg case indicates that "the modern bureaucratic state grew in a more uneven fashion than has heretofore been appreciated" (p. 86). That observation is based on events before the Thirty Years War (1618-1648), but it is a theme that Vann returns to later.

By the eighteenth century, a bureaucratic ethos had emerged in the Württemberg civil service according to Vann. The ducal household was gradually separated from the public treasury. Officials came to see themselves as public servants and not ducal servants. To them office was not a property, but a vocation based on service, thrift, discipline, and work. If that account makes these officials sound like members of the bourgeoisie, it is because that is the conventional account. But Vann's research in Württemberg indicates that these civil servants were aristocrats more often than not. "Their participation in" the development of the professional bureaucracy "warns us against making too close an association of bureaucratic growth with an upwardly mobile, urban, middle class group" (p. 196).

Equally important is Vann's argument that Württemberg absolutism was not a conscious plan of state building as is sometimes said of French absolutism. In Württemberg, it was a maneuver in the constant conflict between the crown, the estates, and the commons. Vann clearly demonstrates that the desire to imitate the Versailles of Louis XIV was one of the duke's many motivations to centralize authority and undercut the estates. The drive of Württemberg's monarchs to reform against the resistance of the estates occasioned Hegel's essay on Württemberg politics in 1815. By that time, Hegel notes, Napoleon had made a few reforms of his own in Württemberg, remarking to a Stuttgart politician that "I have made your master a sovereign, not a despot." On Vann's showing, a great many citizens of Württemberg, including some of those masters, had been working to that end for years.

*Bureaucracy, Education, and Monopoly* consists of comparative case studies of the civil service reforms in Prussia of Stein and Hardenberg after 1806 and in England after the Trevelyn-Northcote report of 1854. Mueller's argument is that because of the reforms, certain strata in both countries monopolized access to the public service through education. Setting aside the functionalist explanation in his introduction, Mueller favors conflict theory. According to conflict theory, the reforms were the outcomes of a struggle, not the inevitable outcomes. In Prussia, Mueller concludes, the rising bourgeoisie secured a monopoly on bureaucratic offices because education became the main criterion for selection. Of course, some aristocrats were the architects of these reforms. Hardenburg is said to have referred to these wide-ranging reforms as the revolution from above. The reforms were embraced by some aristocrats and by the king as the means to modernize Prussia in face of the challenge of revolutionary France. The numerous universities in the realm had long been the province of the bourgeoisie, as the army was of the aristocracy. In this case, the association of the bureaucracy with the bourgeoisie is borne out.

It was a different matter a generation later in England. There the aristocracy retained its monopoly on bureaucratic office by promoting education as the criterion of selection and recruitment. There were only two universities in England, and they remained the private property of the aristocracy. If all aristocrats were not educated, all who were educated were aristocrats. Political patronage and not reform, Mueller argues, would have made the bureaucracy more representative of the populace more quickly. Extensions of the franchise in the period would have meant that politicians securing their seats would have had to have appointed commoners. This argument obscures the relationship between the reforms of the bureaucracy and the wider reform movement of the era. At the time of the first English Reform Bill, an observer like Hegel had already concluded, as did John Stuart Mill, that the application of education as the criterion for civil service recruitment and the extension of the franchise were cut from the same cloth. But if that were true in England, Hegel would have to explain why it was not true in Prussia. Mueller concludes that *carrieres ouverte aux talents* was not advocated as an end in itself in England, or in Prussia for that matter. Nonetheless, it may have been a good in itself. Mueller closes by emphasizing that the English and Prussia roads to meritocracy had nothing to do with democracy. In England the aristocracy used merit to cling to office, while in Prussia the autocracy used merit to shore up its rule. In both countries whenever there was a conflict between status and merit, the powers that were always favored status. Consequently, the changes that occurred were neither inevitable nor impersonal. There was much stopping and starting, and the decisions of particular individuals often did much to advance or retard the cause of reform.

M. W. JACKSON

*University of Sydney*

**Politics and Dependency in the Third World: The Case of Latin America.** By Ronaldo Munck. Zed Books, 1984. Pp. x + 374. $35.50, cloth; $13.95, paper.)

Before plunging into this volume in Zed's Third World Series, the outsider might consider a guide to the radical literature: Paul Attewell's "sociology of knowledge analysis," *Radical Political Economy since the Sixties* (Rutgers University Press, 1984).

Attewell maintains that radical scholars pursue three competing tasks: to maintain the Marxist paradigm, to analyze current events, and to engage in a "moral-evaluating critique." His book is too recent to have classified Munck, but Munck's purpose is clearly to incorporate the usable insights of dependency and world-systems theory into the Marxist paradigm—and then apply the result in what he considers an empirical fashion by fitting it to the past, the present, and future possibilities.

That is, Munck's version of the paradigm. He cites bourgeois writers, the ECLA school, dependency theorists, world-system aficionados and an assortment of known and lesser Marxists.

Marx, Lenin, Luxembourg, Trotsky are generally quoted favorably, the others when they fit (and debated when they do not). His analysis is meant to be applicable to the Third World, a concept which itself is redefined, but there are few references to areas other than Latin America, and there mainly to Argentina, Brazil, Chile, Central America, and Cuba.

Munck's review of events in Cuba perhaps conveys the flavor of the whole. Cuba can go either way—further along the bureaucratic road or toward more genuine socialist democracy. The moral-evaluatory critique: For Cuba's bureaucratic, less than democratically socialist direction blame the pre-Castro inheritance, reliance on the Soviet Union more than Soviet pressure, and Castro's choices. Moreover, more needs be done for women: Patriarchal relations persist.

Back to dependency theory: Where is it useful, where not? Dependency writers provide a critique of nonradical modernization and development theories (good) but have tended to an abstract conception of the world economy (bad). "At its best," this school "points toward historical analysis focused . . . on social classes" (p. 35). Munck surveys stages of "dependent development" in third-world economies in their changing relations to the central capitalist economies. But external factors need be focused, however, on class relations and struggles in the Third World itself, however externally conditioned.

Brief histories of urban and rural social movements follow—labor in Argentina and Brazil, peasants in Chile—to demonstrate that empirical analysis is properly focused, not on classes in isolation, but in relation to others, that is, class struggle, actual, incipient, or thwarted. This section, together with the next, "Politics," comprise the largest part of the book.

The section on politics serves to illustrate neo-Marxist and non-Marxist concepts worked into Munck's schematic histories: *comprador* regimes (Central America) with their enclave economies and personalist states; nationalist and military regimes in Peronist Argentina and since employing the concept of bureaucratic authoritarianism; and Brazil as a more dependent capitalist state than Argentina with a smaller and less activist working class, in a state consecutively oligarchic, Bonapartist, and military. Each history examines specific classes in relation to others and in specific circumstances, but the state is not viewed as a mere epiphenomenon of a static ruling class.

Who will want to read this book? It is meant to provide a Marxist mode of analysis broadly applicable to the dependent capitalist countries of the Third World, but the reader will have to look elsewhere for extended application to Africa and Asia, to which Munck makes only few references (which are hard to locate without an index). Those of us who do not ourselves usually plow Marxist fields might well profit by a reminder that we do not live in a classless world and that class still has explanatory clout, even in socialist countries.

ELISHA GREIFER

*Northern Michigan University*

**Communism and the Politics of Inequalities.** Edited by Daniel N. Nelson. (Lexington, Mass.: Lexington Books, 1983. Pp. ix + 291. $30.00.)

Daniel N. Nelson, prolific author as well as editor of works on East European politics, has assembled here a group of comparative and case studies dealing with political inequalities in communist states. This is an important topic, yet the resulting volume falls short of meeting its ambitious objective, that being "to address the political antecedents, consequences, and correlates of socioeconomic inequity in communist states" (p. vii). It seems to me that of the probable reasons for this shortcoming, the most basic is that the concept of "inequality" is perhaps too broad and unwieldy. Despite its apparent and intuitively appealing promise of pulling together the essential elements of communist political systems—the official Marxist-Leninist theory, public sentiments of legitimacy, economic performance of these systems, and communist international relations—it at once explains too much and too little. Further undermining analysis is that here the concept is neither developed so as to fit into a theory nor placed satisfactorily in a theoretical (and hence explanatory) context. The sole exception is Susan Woodward's chapter on Yugoslavia, which sensibly links inequality with political instability. Except for a passing reference to Ralf Dahrendorf, there is no effort either to call into service, or else to refute, the theory of class conflict. This is a strange omission in a book on political inequality. There is a similar passing reference to Karl Deutsch, yet no appropriation of theories of ethno-politics to help explain ethnic relations. Consequently, the key term *inequality,* although adequately defined, is used more as a common starting point for the various contributors rather than as a concept in the strict sense. It is understandable that the contributors focus on different facets of (and use a variety of) approaches to study this aspect of communist politics, but it is also understandable that no palpable answer emerges to the central question concerning what "the politics of inequalities" are.

It is gratifying to see scholars of communist systems attempting to integrate elements from other fields of political science into their approaches. In this volume, for example, Valerie Bunce uses dependency theory to explain the effect of international inequalities on domestic, internal ones; Dan Nelson uses the development theorists' notions of "crises and sequences"; and John D. Robertson and David M. Lampton draw on the public policy literature to analyze Soviet and Chinese welfare policies. This is commendable, because thus we are usually offered better explanations, not just the same old wine in newer, fancier bottles.

There are limitations to this integration, as the authors themselves concede more or less explicitly. The connection, for instance, between dependency and inequality of Eastern Europe with regard to the USSR is not fully established or demonstrated by Bunce, although it is plausible enough. Her conclusions, like those of most of her co-contributors, are very cautious. Resort is made to metaphorical terms to aid in the explanation. Two of the writers concede (pp. 160 and 184) that the end of their chapters should properly be but the beginning of research on the matter in question. Clearly, this book is an advance on what has been done; it is not by any means the last word.

The most systematic essay is by Cal Clark, although more straightforward prose would have helped to convey meaning better. Does Yugoslav decentralization, after all, mean more or less inequality, or more or less equality? What *does* Yugoslavia illustrate? No such ambiguity haunts Jack Bielasiak, whose chapter on Poland is the most interesting and satisfying of the volume. This is because he not only measures economic inequalities, but he also demonstrates by reference to public opinion surveys that these are perceived as significant. Hence inequality is shown to be politically relevant, something few of the other contributors are able to do, usually because of lack of data. Most of the other authors are able to describe the existing inequalities statistically, but are less well able to assess their political effects.

Other contributions are by Donna Bahry on the political versus economic inequalities of Soviet republics, Mary Ellen Fischer on ethnic relations in Romania, and Sharon Wolchik on the mixed effects of federalism upon relations between Czechs and Slovaks since 1968.

The strict balance between genders among this book's contributors, incidentally, is welcome evidence of an important kind of equality among the practitioners of comparative communist studies, even though a comprehensive explanation of this vital relationship in the countries they explore as yet eludes their grasp.

BOHDAN HARASYMIW

*University of Calgary*

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**Ulster's Uncertain Defenders: Protestant Political, Paramilitary, and Community Groups and the Northern Ireland Conflict.** By Sarah Nelson. (Syracuse, N.Y.: Syracuse University Press, 1984. Pp. 219. $32.00.)

Although social scientists have swarmed over Ulster since 1968, the urban Protestant working class has been rather neglected as a subject of systematic study. This lacuna stems from often quite mutual suspicion, apprehension, and distaste between roving researchers and loyalist communities and organizations. In the absence of careful fieldwork inquiries, the image of Protestant workers in the literature has ranged from that of an undifferentiated mass of bestial bigots (the "authoritarian personality" with a Northern brogue) to the optimistic portrait, drawn by a few Marxists, of a befuddled class recuperating—albeit in violent fits and stumbling starts—from a sectarian epidemic of false consciousness. Dupes, psychopaths, and nascent class-conscious actors are not so difficult to find (or to exaggerate) in the six counties. What is lacking are comprehensive analyses, based on intensive grass-roots contact, which illuminate how loyalist groups come to perceive political events, define their stances, and deem their actions rational.

In *Ulster's Uncertain Defenders,* Sarah Nelson provides a welcome and well-researched examination of the complex tissue of interactions among political parties, paramilitary groups, and (mostly) new community organizations in Protestant neighborhoods of Belfast from 1969 to 1975. The authors lays special stress on the strains to which Orange ideology was subjected "because any general explanation which analyses economy and social structures must be able to fit the subjective reality into its framework, just because social consciousness is the active process in determination of political struggles" (p. 10). In this effort, Nelson produces a worthy companion piece to Frank Burton's earlier probe into the "politics of legitimacy" in republican neighborhoods of Belfast.

In Part 1, covering 1920 to 1969, Nelson appraises the key elements of social ideology and state structure that contributed both to hard-line attitudes toward Catholics and to deferential behavior with regard to the elite in charge of the reigning Unionist party. In Parts 2, 3, and 4, Nelson focuses on three events that tore the

Unionist bloc asunder: the arrival of British troops in August, 1969, the imposition of direct rule in March, 1972 (and its aftermath), and the mass loyalist strike (abetted by some bullying) in May, 1974 that brought down a promising experiment in power-sharing.

Beneath supremacist slogans and despite favorable discriminatory practices, internal dissention simmered throughout the heyday of Stormont rule, but any independent assertion of working-class interests was easily checked. The growth of even the mildest-mannered form of progressive political action was (and is still) precariously reliant, as Nelson notes, on "low cross-border tension" and a "belief that the cake could be made bigger all around, not redistributed in a way that left the majority with less" (p. 47). Conservative Unionist representatives proved unresponsive to the local grievances of loyalist communities on matters of amenities and services—and these areas, as any visitor can attest, are hardly havens of luxury (hence the concern regarding "redistribution" of already frugal living standards).

The Civil Rights movement galvanized a "dual discontent" felt by Protestant workers toward the regime; O'Neill's conciliatory policies of the 1960s seemed a betrayal, and, further, the existing structure appeared to have done little on their own behalf. Nelson's extensive interview material illustrates vividly the deep confusions, resentments, and lack of self-confidence that daunted the "tentative steps toward self-assertion" by breakaway loyalist workers. Paisley's Democratic Unionist party offered an *Escape from Freedom* refuge for many; the Ulster Defence Association (UDA) and Ulster Volunteer Force (UVF) became homes for sectarian assassins and some would-be social reformers whose struggles would send those organizations careening in one direction and then the other (several times); and community action groups absorbed the energies of others. Although Nelson clearly sees the community action groups as a hopeful development—a benign crucible in which to develop political skills and to define progressive goals—the optimism is guarded. All groups are "vulnerable to traditional pressures," and Nelson also acknowledges the cessation of cross-community ties (i.e., with Catholic groups) since the late 1960s. The book provides fair, concise, and sober judgments regarding the range of people attracted to various groups, the constraints under which they operate, and the often subtle relationships among them—particularly between local communities and the paramilitary organizations. One wishes, however, that Nelson would give at least "ballpark figures" to account for the number of breakaway loyalists who joined each group.

*Ulster's Uncertain Defenders* aids understanding of the ways loyalists maintained the "integrity of their self-images" in the face of contradictions between sub-state violence and a professed devotion to law and order (*whose* law and *whose* order are, of course, the stakes of the conflict), of the motives of the actors, and of their attachment to rather tenuous identities. (One group argues in an interview [p. 114] that they are "even more loyal than most Britons, because they accepted far lower wages than mainlanders!") Overall, Nelson has written an extremely useful book, and one hopes that a second volume dealing with the period from 1975 to 1984 is in the works.

JOHN KURT JACOBSEN

*University of Chicago*

**Interest Groups and the Shaping of Foreign Policy: Four Case Studies of United States African Policy.** By F. Chidozie Ogene. (New York: St. Martin's Press, 1983. Pp. 224. $27.50.)

The visit of Bishop Desmond Tutu, the second South African to become a Nobel laureate, to the White House and American concern with famine in Africa suggest that African issues do obtrude upon the American foreign policy process and evoke official and unofficial responses from elites and attentive publics alike. Just how such issues are placed upon the public agenda and the roles various American interest groups play in shaping and resolving such issues constitute the central focus of this book. Ogene, who is a Senior Research Fellow of the Nigerian Institute of International Affairs and a former member of the Nigerian foreign service, was educated in the United Kingdom, the Netherlands, and the United States, where he taught political science at Central State University in Wilberforce, Ohio.

His book, which is a revised version of his 1974 doctoral dissertation at Case Western University, is arranged in a crisp, logical fashion and is intended for those political scientists interested in comparative and international politics as well as for Africanists. The research is centered upon four particular case studies, two of which are devoted to Southern African concerns (economic sanctions against Rhodesian chrome and import quotas for South African sugar), the third concerns the Nigerian Civil War, and the fourth involves the near-disintegration of Zaire in the period immediately following its independence from Belgium. Ogene introduces his study with three hypotheses, only one of which focuses upon the specific region of Africa. The other two refer to the expected utility of tangible and nontangible (symbolic) concerns of those interest groups involved in the foreign policy formation process and the relative weight to be assigned to external (situational) foreign policy concerns and internal interest group assets and liabilities.

Ogene then explores briefly the English-language literature on the subject of interest groups and foreign policy before moving on to the task of providing operational definitions for the concepts used in his inquiry. Both in the introductory and concluding chapter he comes to grips with the methodological problems concerning the study of influence and the utilization of case studies. Although he provides no arrestingly innovative solutions to these nagging methodological concerns, Ogene is well aware of the limitations of his study and recognizes that his is not the definitive work on Africa and the American foreign policy process.

Even though he confirms his first hypotheses "that interest groups exert significant influence on U.S. foreign policy on African issues" (p. 191), Ogene does not enable the reader to determine just what "significant" means either in nominal or ordinal terms. He does, however, provide a thoughtful taxonomy of influence derived from his four case studies. His second principal finding is that tangible and symbolic interests are not mutually exclusive categories in terms of group activity. He indicates that "groups with interest in symbols were influential where there were no groups with material interests opposing them or where they could supplement their case by arguments showing that tangible resources could be secured or fostered by their policy preferences" (p. 195).

Ogene's third conclusion is that group-related and situational factors must be considered together, and that the former does not weigh more heavily than the latter. He provides a useful manner of looking at the different types of interest groups operating in the foreign policy setting and suggests how they are linked to the mass public, to external actors and the foreign environment, and to the foreign policy decision makers and their decisions.

This book is a competent treatment of a significant topic in foreign policymaking. However, the revision of Ogene's dissertation did not entail much updating of his sources; only four entries in the bibliography were published after 1974. Moreover, the total number of his interviews in Washington is not given, nor are the reasons for his selection of interviewees made clear, even though he makes explicit the methodological pitfalls involved in using interviews for his study. Ogene's selection of American newspapers as data sources is exceptionally limited and excludes those that cater to black American audiences. Finally, the care with which the selected bibliography was assembled leaves much to be desired.

RICHARD DALE

*Southern Illinois University at Carbondale*

**Births, Deaths and Taxes: The Demographic and Political Transitions.** By A. F. K. Organski et al. (Chicago: University of Chicago Press, 1984. Pp. xii + 161. $18.00.)

This book suggests that "the larger the scale of government, the lower the rate of childbearing and the lower the rate of mortality" (p. 2). One test of this hypothesis is a highly disaggregated study of *how,* in a particular spatial/temporal context, the many aspects of government influence the many aspects of demography. This single-case approach sacrifices breadth for depth. Organski et al choose to test the hypothesis with a highly aggregated multiple case approach: They regress, in a cross-national sample of nations, birth rates (Tables 3-6) and death rates (Tables 7-10) on a tax-based measure of political development. Their test supports their hypothesis. By boldly sacrificing depth for breadth, Organski et al have produced a most innovative and provocative work.

But how do the authors relate "taxes" to "political development"?

> We establish a maximum possible level of taxation for developing and for developed countries. . . . We index the level of political capacity reached by taking the fraction of total taxes over GDP. After the maximum and taxes are adjusted on the side of expenditures for policy preferences and on the supply side for economic differences in the tax base, we subtract the adjusted taxes from the adjusted maximum. This is the estimate of political costs we shall use in our analysis as the measure of the independent variable, political development. (p. 79)

Organski et al are to be commended for raising a set of theoretical linkages that are at the heart of comparative analysis: These linkages run from the state's power in the international arena, to the political development or the expansion of the state, to the political performance or the political effectiveness of the state, to the state's capacity to extract resources, and finally to the (marginal) political costs the state faces in containing internal resistance to the whole development process.

The authors' approach to the fiscal politics of tax revenues is not, however, completely satisfying because they provide only vague labels rather than conceptual definitions. It is the *conceptual* as much as the *operational* significance of the key terms, however, that are in dispute. Thus, as one reads Organski et al's careful reasoning about how "political development" is assessed via "taxes," one cannot help but recall the equally well-reasoned counterarguments to the whole political development tradition: capabilities are not activities; more activities are not necessarily better activities; and better is inherently a normative and not a positive issue. Thus the nagging feeling remains that the political development concepts themselves are too illusive, intangible, and imprecise to guide theoretical and empirical thinking.

And how do the authors relate "taxes" to "births and deaths"? The authors' approach to the demography of taxes is, unfortunately, not completely satisfying either. The theory of the state offered to account for the impact of politics on demography is unspecified. The effects of the expansion of central government on fertility and mortality are purported to be mostly "indirect" (p. 4), "unintended" (p. 118), "unmanipulable" (p. 15), "unconscious" (p. 13), and manifested in so many different ways (pp. 27-34) that "the list of such connections can be made long indeed" (p. 4). The theory of the state offered is also eclectic. It is partly functionalist: "It is generally agreed that any state must perform three principal functions . . ." (p. 45). The theory is also partly crisis-oriented: The performance of a government is best evaluated during the crisis of total war (pp. 67-70). And the theory is partly rational choice/predatory: "All governments need or at least want all the resources they can get" (p. 110).

Hence, the key problem with this work: Although the authors' evidence establishes the breadth of their central proposition, the theoretical linkages between births, deaths, and taxes are so amorphous and complex that the finding itself needs an in-depth explanation.

What went wrong with this study of how taxes stand between political development, on the one hand, and demographic change on the other? In sacrificing depth for breadth, the authors have formulated but not dissected the problem. Consider their index. The procedure used to measure political development assumes that (1) each nation's political performance can be reduced to a single number because (2) a single unidimensional index of political performance, which is constructed out of the "indicators" (p. 63) of the key concepts, can represent the concepts themselves. But the really interesting issues turn out to be *how* the index was constructed from its components. And consider their equations. Two (essentially) bivariate regressions, linking taxes to births and deaths while controlling for region and GDP, are estimated. But the really interesting issues turn out to be *how* politics affects demography. Rather than proposing complicated and ad hoc justifications for estimating a very small number of key parameters, what is needed is an overall model, consisting of a series of interrelated equations, to get at the political and demographic development process.

In summary, Organski et al have raised the issue of a linkage between political and demographic change, but they have not really opened the black box and offered us a convincing and precisely defined causal mechanism of how the development process operates. Their work takes the first crucial step by examining the hypothesis in all its breadth. Organski et al do raise all the right questions. However, as analysts become concerned with the hypothesis in all its depth, new answers that build on Organski et al's approach will have to be offered.

MARK LICHBACH

*University of Illinois at Chicago*

**Politics and Change in East Germany: An Evaluation of Socialist Democracy.** By C. Bradley Scharf. (Boulder, Colo.: Westview Press, 1984. Pp. xii + 220. $27.00, cloth; $11.95, paper.)

While the German Democratic Republic (GDR) has been the object of growing attention from scholars in recent years, locating it in an appropriate framework for analysis continues to be problematic. Is the GDR to be viewed as little more than an obedient instrument of Soviet foreign policy, as one of several East European nations wrestling simultaneously with the problems of economic development and national legitimacy, or as a poorer and smaller communist brother of the German Federal Republic? Or is it, in spite of its communist political system, more usefully regarded as an advanced industrial society comparable in many ways to the capitalist welfare states of Northern Europe?

In this survey and interpretation of politics and society in the GDR, C. Bradley Scharf tries to view it especially in terms of the last perspective. There is much to be said for such an approach. The GDR is the most prosperous and technologically the most advanced of the communist states. It now has the largest relative volume of trade with the West of any of the Soviet Union's East European allies, which helps to make it in turn the leading supplier of advanced technology to the USSR and the rest of the bloc. Its program of social benefits, given special emphasis under Erich Honecker, offers a range of economic protections to its citizens that in some ways invites comparison to those provided by the Scandinavian states. As Scharf notes, many of the GDR's problems—environmental deterioration, youth unrest, the pressures on traditional attachments reflected in high divorce and suicide rates—are also those of advanced capitalist societies.

The heart of Scharf's book is to be found in the chapters dealing with the efforts of the regime to promote social integration and with the continuing reality of social differentiation and discontent. The Marxist-Leninist model set forth by the GDR is one of a solidary society in which class antagonisms have given way to mutual cooperation, widespread citizen participation functions not to further particularistic interests but to advance the collective good, and pervasive childhood and adult socialization helps unite people and the ruling party in the building of a new and superior socialist way of life.

In fact, GDR citizens find themselves tempted by the blandishments of consumerism and bourgeois democracy transmitted nightly by West German television, while also facing an internal tension between the regime's ideals and its achievement-oriented practice. The GDR's claims to be creating a more humane and cooperative society do not fully comport with the heavy emphasis on performance—on the job, in sports, in social activism—in its reward system. Visible differences in access to privilege, inconsistent policies of repression and relative permissiveness in matters of personal and literary expression, and the search of many citizens for private "niches" (Günter Gaus) that offer an escape from the demands of the state all tend to subvert the integrative ideal.

Some parts of this book—the chapters on economic and social policy, for example—are admirably researched and executed. The pivotal chapters on social integration and differentiation, however, occasionally drift into excessive speculation and neglect important East and West German studies that might better illuminate the subject. Matters that would seem particularly important for understanding the GDR's attempts to implement its integration model—the actual role played by ideology in social management and the importance of militarization in the socialization process—receive insufficient treatment.

Overall, however, Scharf has given us a thoughtful and frequently novel analysis of a society and political system which is likely, as a model of communism in an advanced industrial setting, as an actor in international politics, or both, to have significant influence on the future of both Eastern and Western Europe.

THOMAS A. BAYLIS

*University of Texas at San Antonio*

**Minority Rights: A Comparative Analysis. By** Jay A. Sigler. (Westport, Conn.: Greenwood Press, 1983. Pp. x + 245. $29.95.)

Sigler has written a creditable survey of the status of minority rights throughout much of the world. Some comparativists will lament that the work is not really a comparative analysis: The data presented are not compared either quantitatively or in terms of a verbal typology. However, it is refreshingly free of jargon and thus would be a useful textbook for an undergraduate course in comparative rights. (A cheaper, paper edition would be appreciated.)

Sigler begins by defining *minority* and rightly insisting that there is a difference between individual and minority rights, and that liberal theory has emphasized the former to the neglect of the latter. (For example, to Sigler, not only freedom of speech, but also the right not to be discriminated against on account of, say, color, is an "individual right," whereas the right of a minority group to use its own language is an example of a "minority right.") Chapter 3, by James Muldoon, is the most original feature of the volume, providing a history of the development of group rights. Its basic conclusion, that freedom of religion ultimately appeared in Europe because the contending faiths could not destroy one another, is strikingly close to the thesis of writers such as Barrington Moore that democracy developed in certain countries because clashing socioeconomic groups found themselves bogged down in stalemate.

Chapter 4 discusses the protection of minority rights by international agreements, which Sigler concludes is very limited. Chapter 5 mentions instances where the minority has dominated or does dominate the majority, with most time being spent on European colonialism and the Republic of South Africa. Chapter 6 deals with what Sigler terms *minority rights arrangements* in Belgium, India, Switzerland, and the USSR. Subsequent chapters treat affirmative action programs in the United States and the United Kingdom. A brief juxtaposition of the U.S. and UK experiences is the only real "comparison" in the work. The final chapter sums up earlier themes, sensibly points out that demands for independence by minority groups are probably just going to strengthen the position of those who oppose minority rights claims, and provides (under the rubric of "a provisional theory of minority rights") a list of certain rights due minority groups.

As will any global survey of political phenomena or problems, the book presents case studies that merely skim the surface in providing information and analysis. As an information source, it is inferior to Kurt Glaser and Stefan T.

Possony's *Victims of Politics* (Columbia University Press, 1979), which Sigler apparently has not read. Sigler has worked hard, nonetheless, some of his sources need to be updated. For example, in his affirmative action chapter he declares (p. 136) that the "Supreme Court must ultimately confront the group rights issue"; no mention is made of the 1980 case of *Fullilove v. Klutznick.*

The manuscript also contains what will strike the average reader as contradictions. For example, on page 127 Sigler says that "of all the unresolved internal problems of the Soviet Union, the nationalities problem appears most serious." On page 129 we read that there "is no apparent sign of a severe nationality crisis in the Soviet Union . . . defiance [by minority groups] is rare and violence even rarer." On page 145 Sigler claims that "affirmative action in India is a controversial policy still, in spite of decades of experience in developing it. . . . In 1981 the reservation of 25 percent of the places in medical schools for untouchables set off riots in Gujarat State that lasted intermittently from January to April." Yet at the bottom of the same page he optimistically contends that "it is not impossible to blend the nondiscrimination ideal with an affirmative action policy. A few nations, most notably India, have managed to do so." On page 7 he indicates that "religion may not be enough to bind a group into a coherent minority, although small sects . . . frequently are perceived as minority groups." Despite this implication that there is something dubious about considering religious groups as "minorities," quite a few of Sigler's pages are devoted to the rights of religious groups.

Some readers will be annoyed by other facets of this essentially solid book. For example, in his discussion of church-state separation in England, Sigler omits the significant fact that denominational schools there get considerable public financial assistance. On page 194 he maintains that "there is a difference between joining the AFL-CIO and being a Jehovah's Witness, and this difference accords more rights to the religious group member than to the unionist." It is unclear whether this is a statement of fact or value, and no justification is provided for it.

DANIEL C. KRAMER

*College of Staten Island, City University of New York*

**The State as Terrorist: The Dynamics of Governmental Violence and Repression.** Edited by Michael Stohl and George A. Lopez. (Westport, Conn.: Greenwood Press, 1984. Pp. viii + 202. $29.95.)

This volume represents a first step toward filling a long-standing need in the terrorism literature for conceptual analyses to deal with the use of violence by governments, and it offers some promise of moving the analysis of terrorist violence beyond the bias in favor of incumbent authorities that characterizes present conceptual frameworks. Students of terrorist violence learn very quickly that the adage "one man's terrorist is another man's freedom fighter" is as true in the literature as it is in the political arenas. Ideological diatribes and narrowly focused analytical perspectives on terrorism fill library shelves, as do analyses that lack any real conceptual or theoretical underpinning.

In the Introduction, Stohl and Lopez note the fact that the systematic study of terrorism is a relatively recent development and that official or state terrorism, including vigilante terrorism, has been an overlooked aspect of the literature. They go on to note the failure to arrive at a workable legal definition that would permit international action to outlaw terrorism and conclude that the problem lies in focusing on the identities of the terrorists and their causes rather than on the nature of the acts themselves.

In terms of the chapters that follow, John McCamant offers an analysis of the iconic models that have characterized social science research in recent decades and concludes that the interest group, systems, Marxist, dependency, elite, and geopolitical models and theories have been ill-equipped to deal with the phenomenon of political repression. The remaining chapters deal with terrorism from a variety of orientations. Michael Stohl discusses the international aspects of state terrorism from the applications of terror in diplomacy and nuclear strategy to the uses of terror by government agencies in clandestine attacks on foreign governments or persons and the subsidizing of surrogate terrorism.

George Lopez then offers a "scheme for the analysis of governments as terrorist," including the precipitants, the institutionalization, the ideological bases, and the character of state terrorism, to include the types of violence, the targets/message complex, and the technologies available. The beginning point for much of his analysis is the state's inherent capacity for coercion and the relative ease with which it can use that power to repress opposition and dissent.

John W. Sloan examines state terrorism in Latin America, providing an overview of the problem, the precipitants, particularly elite dominance, and the influence of political culture on the choice of repressive policies. David Pion-Berlin examines state repression in Argentina in terms of the impact of economic factors, including land reform and labor unions, on the repressive activities of the state. Jim Zwick looks at the links between increased militarism in the Philippines and the propensity of the Marcos regime to use repressive violence. Using theories and models of revolutionary violence, Robert A. Denemark and Howard P. Lehman analyze the costs that may be exacted because of continued repression in South Africa. Frederick D. Homer describes and explains government terror in the United States in terms of "containment policy" or the tendency to reduce real or imagined challenges to government authority, public safety, or civic order, through persuasion, intimidation, or violence. Lopez and Stohl conclude with a proposed research agenda and a bibliographic essay.

The volume is uneven, ranging from essays to empirical analyses, from country studies to epistemological theories. Nonetheless it attempts an important conceptualization. The terrorism literature can be frustrating for one looking for conceptual clarity and objective analysis. The serious student will find this volume useful and stimulating.

WILLIAM L. WAUGH, JR.

*Georgia State University*

**The Immortal Ataturk: A Psychobiography.** By Vamik D. Volkan and Norman Itzkowitz. (Chicago: University of Chicago Press, 1984. Pp. xxv + 374. $37.50.)

The new genre of psychologically sophisticated biography, of psychobiography, is developing as rapidly as the Third World. What is so special about this new book is that, like Edgar Snow's *Red Star over China* (1938), it relates a remarkable people to a remarkable leader in the Third World. Like China, Turkey has had a long and proud past. The Ottoman Empire began in the fourteenth century, peaked when Turks almost took Vienna in the sixteenth century, declined, and at last expired when the new republic was founded in 1923. Often rightly renowned for their ferocity, Turks as late as 1915 began to relocate about 1.75 million Armenians, hundreds of miles from their homeland on the Russian frontier. Close to half starved or were slaughtered en route.

Such aggressive efforts stopped after the war. Mustafa Kemal Ataturk is the man who singly was most responsible for the Turkish victory in 1915 over the British at Gallipoli and for the defeat in 1922 of the Greek invasion that penetrated nearly to Ankara in the heart of the once overweening Ottoman Empire. But he then turned his people's attention to internal reforms. By Romanizing the alphabet he facilitated literacy; by denationalizing the Islamic priesthood he diminished the political hold of that religion. He helped Turks manumit themselves from their past while he remained a pragmatist and a teacher.

The most remarkable element in his domination of the country was the degree of restraint in the use of force; the scale of repression there was perhaps much less than in Germany or the Soviet Union in the 1930s.

This book is a major effort at explaining how modern Turkey came about and how Ataturk ("father of Turks") became its founder. Modernization had already commenced in the last century of the Ottomans. In the latter half of the nineteenth century, there was a rapid rise in population. But the economic and political elite—seemingly like ancient regimes in all developing nations—regarded the public as ignorant instruments of their efforts to aggrandize themselves.

Kemal was the son of a literate, entrepreneurial, and free-thinking lumber merchant and of an illiterate, strong-minded, devout young Muslim. He was overprotected and adored by his mother, even more after his father died when Kemal was seven and his mother only 27. He became prodigiously active sexually (and in every other way) but repeatedly in his public career said "I am in love with my mother."

The authors reiterate themes of mother-son ties, in Ataturk's passionate devotion to moving his backward country forward. At the same time, they report his enormous energy and his very high opinion of himself—when he was not in one of his occasional periods of profound depression. It is hard to overestimate his passionate fidelity to his people.

He may best be characterized by an entry he made in his diary while recuperating at a spa in Germany during the war:

> Why, after my years of education, after studying civilization and the socialization process, after spending my life and my time to gain pleasure from freedom, should I descend to the level of common people? I will make them rise to my level. Let me not resemble them: they should resemble me. [Quoted on p. 104.]

To understate the relationship, he never tried to distance himself from people, whether in personal or political affairs.

This is an altogether absorbing book: It gets into the psychodynamics of a developing nation and leader that have in many ways been at once unique and universal. Indeed, both the strong and the weak points of the book relate to Turkey's uniqueness. The strength lies in the effort to relate modern Turkey to its past, and to relate Mustafa Kemal Ataturk to both his own past and his vast public following. The weakness derives from the impression the authors give that Turkey and he are indeed *only* unique and not comparable to other nations and leaders.

There is not enough consideration of the social history of these hard-formed and conservative people, whose numbers have tripled in the two generations since the Republic was founded. Nor is there adequate consideration of the new demands they were making for at least two things: recognition by their own leaders and by other nations of their dignity as human beings and the right to participate in significant political action. The authors tend to use straight historical analysis of gross national events that are more complex than battles and to use conventional Freudian concepts to explain a personality that is more than just Oedipally complex. There is not much discussion of the modernization process as Durkheim, Marx, and Daniel Lerner examined it.

Napoleon was one of the earliest modern rulers to understand and exploit ordinary people's wish to get respect and get in on the action. In a strange way, so did Hitler. The difference between these two rulers and Ataturk is that he helped his countrymen to get politically involved and to gain respect without turning to genocide, slave-labor camps, and war or subversion abroad. Other nations, both developing and developed, should be so lucky.

JAMES CHOWNING DAVIES

*University of Oregon*

**Political Violence, Crises, and Revolutions: Theories and Research.** By Ekkart Zimmermann. (Boston, Mass.: G. K. Hall, 1983. Pp. xvi + 792. $45.00.)

Ekkart Zimmermann's encyclopedic assessment of contending theoretical approaches and cross-national empirical studies of political violence represents an ambitious attempt at analyzing three major areas of concern: systemic crises, military coups d'etats, and revolutions. The mammoth character of this work, its demonstration of both depth and breadth with regard to the literature on political violence, clearly establishes it as the most comprehensive source of knowledge in this field of study. Yet the content of the book is somewhat deceptive. The actual text is only 434 pages, followed by 354 pages of notes, bibliography, and index. Nonetheless, Zimmermann makes a formidable contribution at the levels of theory, methodology, and empirical research.

This study begins with a preliminary examination of conceptual problems, experimental studies of aggressive behavior, and theories of social comparison processes (group theory). Zimmermann adopts a working definition of political violence based upon H. L. Nieburg's behaviorally oriented "bargaining model" while rejecting social-psychological explanations of aggression (both frustration-aggression and social learning theories). Following this, he launches into a detailed analysis of the cross-national quantitative approach to the study of political violence. Here the work of Ted Robert Gurr and Douglas Hibbs is paradigmatic for Zimmermann. Gurr's studies of civil strife and Hibbs's research on mass political violence are rigorously dissected, analyzed, critiqued, and praised, with primary attention paid to methodological issues of research design, formal modeling, operationalization, measurement techniques, coding procedures, sample selection, and multivariate analysis. The research of Ivo and Rosalind Feierabend is also reviewed, but their "frustration-aggression nexus" as it relates to systemic frustration is found to be of questionable explanatory worth. Finally, a section on the riots of American blacks during the 1960s is included not only due to the wealth of empirical data on this phenomena, but also to illustrate the many methodological problems encountered in doing political violence research. Throughout this exhaustive account of varying approaches and quantitative studies, Zimmermann impressively combines thoroughness with precision and a keen eye for details.

Systemic crises and their relationship to political violence are approached by way of an evaluation of structural-functionalism and systems theory as well as a critical commentary on most of the mainstream literature on political development and modernization. Leonard Binder's "crises and sequences of political development," the work of Raymond Grew and associates, Seymour Martin Lipset and Stein Rokkan, Gabriel Almond and Sidney Verba, and Samuel Huntington are all scrutinized. Zimmermann's general critique of "the underdevelopment of crises research" (p. 189) is unfair however, because it masks a deeper frustration on his part over the fact that the often-used historical case study approach does not neatly fit into his particular quantitative-behavioral orientation. In the end, Harry Eckstein's political performance decision-making approach and Gurr's causal model explaining "persistence and change in political systems" are chosen as the most fruitful frameworks with which to understand political crises. Huntington's theory of praetorianism informs the chapter on military coups d'etats. Here post-World War II empirical findings are examined and summarized in the form of an elaborate causal model that seeks to represent the synthesis of numerous crucial variables. The work of Huntington, Barrington Moore, and Theda Skocpol provides a broad theoretical background for the cross-national study of revolutions. This examination culminates in a careful look at the empirical work of Charles Tilly on collective violence. Zimmermann is especially critical of Moore, arguing that in the final analysis his comparative-historical analysis of modernizing revolutions proves nothing. The fault here, as with most of Zimmermann's criticisms, is that everything is reduced to a problem of methodological precision. Grand theorizing and statistical manipulations mix very poorly. Continuing with an inquiry into the work of Eric Wolf on peasant revolutions, a brief overview of guerrilla warfare and political terrorism, and a review of George Rude's studies on revolutionary crowds, a "raw causal model of revolutions" based upon a revised version of Tilly's "power-contending model" is proposed in summation.

Zimmermann is correct in concluding that the subheading of his book should have read "Trends Report," for that is precisely what it is. Although his exposition and critique of a vast array of theories and research is unquestionably a valuable contribution, no new major theory or synthesis is presented. Rather, a strategy of "incremental model specification" is employed; a tinkering with existing approaches, models, and findings culminating in modest reconstructions and a call for more research. Yet his own quantitative approach systematically relegates to the periphery of political violence research the importance of ideological factors as well as the significance of neo-Marxist analyses of systemic crises, revolutions, and counterrevolutions. Also, the phenomenon of contemporary terrorism, worthy of an entire chapter of its own, is dealt with only in a cursory fashion. Despite these shortcomings, Zimmermann's work is a solid effort and should stand for some time as essential reading for the serious student of political violence. With this book as a foundation, Ekkart Zimmermann should continue to make significant inroads in the comparative analysis of social conflict and political violence.

WAYNE GABARDI

*University of California, Santa Barbara*

# International Relations

**Conflict in World Society: A New Perspective on International Relations.** Edited by Michael Banks. (New York: St. Martin's Press, 1984. Pp. xxi + 234. $27.50.)

This book contains a collection of essays in honor of John W. Burton and his World Society approach to international relations. The book is divided into three sections. The first explores the rise of the World Society perspective, discussing the evolution of international theory, the World Society approach as a new paradigm for the study of international relations, and the manner in which theory is molded by the perceptions of researchers. The second examines the structure of World Society, addressing such things as the World Society view of the relationships among international actors, the role of decision makers, conflict in World Society, and psychological processes and crisis prevention and resolution. The final section seeks to apply the World Society approach, investigating subnational community participation in World Society, problem-solving workshops, terrorism and its resolution, national foreign policy behavior, and the interaction between academics and practitioners.

Although the praise for Burton is well deserved, and the essays generally are well written, there are several difficulties associated with this book. The first has to do with its organization and the integration of the essays. Beyond the vague references to World Society that are found in most of the essays, there is little effort to tie the chapters together. There is no introductory chapter summarizing the World Society approach, nor is there a concluding essay that discusses the basic points made in the book and relates them to the World Society perspective. Moreover, only Chadurch Alger, Christopher Hill, and Richard Little are explicit about how the points they discuss relate to World Society. Consequently, the reader is left with a collection of essays that often appear unrelated, either to one another or to the World Society perspective.

A second problem relates to the nature of the essays. In general, each chapter consists of a summary of a selected aspect of international relations; however, the essays report no new research, nor do they either attempt very extensive reviews of existing literature or aim at further theoretical development or refinement of the World Society perspective. Exceptions are found in Alger's clear and concise discussion of his Columbus study; in Little's useful comparison of what he calls the radical, pluralist, and World Society views of international affairs; and in Hill's critique of the World Society approach. Clearly, considerations of space preclude the reporting of extensive research or the attempting of thorough literature reviews, but the failure at least to attempt some theoretical refinement is disappointing, especially in light of the discussion in the first section of the book of the World Society approach as a possible new paradigm for the study of international relations. If the World Society conception is to be considered a useful new paradigm, then surely its advocates must convincingly demonstrate its theoretical and practical superiority to other potential approaches. The discussion of conflict prevention and resolution is a case in point. There is a general consensus among the contributors that a World Society approach is superior to others, but there is no clear-cut indication as to precisely what such an approach entails beyond the vague notions that incorrect perceptions exacerbate conflicts, that increased interactions (of the proper type) among antagonists may be useful, that mediation efforts are valuable, and that a variable-sum view of conflict situations is preferable to a zero-sum view. Such ideas are hardly new and are not unique to the World Society approach. Moreover, Frank Zagare (*Game Theory: Concepts and Applications,* Sage Publications, 1984) has demonstrated that it is incorrect to view variable-sum conflicts as inherently less intense and more amenable to solutions than zero-sum situations.

As it is, this book points to the need to consider alternative approaches to the study of international relations. Future efforts should focus on being more precise as to the unique contributions that this particular approach offers.

JOHN M. ROTHGEB, JR.

*Miami University*

**Economics and World Power: An Assessment of American Diplomacy since 1789.** Edited by William H. Becker and Samuel F. Wells. (New York: Columbia University Press, 1984. Pp. xvi + 474. $35.00, cloth; $10.00, paper.)

It is timely, as we debate the nature of recent American leadership in world politics, to review our past growth, expansion, rise to world prominence, and decline from hegemony. We must understand not only the domestic and foreign economic sources of U.S. power and prestige, but also the way in which economic interests directly affect the nature of U.S. world leadership. Is it true, for example, that short term and domestic economic interests are more likely to prevail in U.S. foreign policy when domestic or foreign economic position is relatively weak?

*Economics and World Power* (which should be titled *Economics and American Diplomacy*) may also indirectly help advance thinking about other important themes in politics. U.S. leaders and coalitions have often used economic interests and objectives to justify noneconomic purposes. This strongly demonstrates the necessity of carefully weighing official statements to sort out economic arguments from real economic motivations, for example in questions of economics and national security. Closely related is the history of how the United States applies economic means to achieve political and strategic ends, including the pervasiveness of "reciprocity" as a guiding principle and diplomatic instrument of U.S. foreign policy. Finally, analysts of international politics, U.S. foreign policy, and comparative foreign policy will also find documentary material of importance for weighing the balances between market forces and government planning on the one hand, and between domestic and international sources on the other.

This is an edited book in the finer sense. It draws on the individual expertise of nine experts, allowing each to focus on his (with an unfortunate neglect of women historians) special historical period in U.S. diplomatic history within the framework provided by the editors through a set of overarching questions. Although clearly focusing on economic influences, the authors successfully carve out a more precisely defined middle ground between the over emphasis of economic factors by revisionists and the under emphasis by realists. It is most valuable not for new findings but for the consistent, careful, and comprehensive treatment, under one cover, of economic influences at all levels of analysis in American foreign policy over some two centuries. For a scholarly volume, it is surprisingly well written. The work does, however, merit a serious conclusion rather than a brief epilogue.

The book's principal flaw is imprecision and redundance in the analytical framework. The editors should have demanded less emphasis in their focusing questions for the authors on disproving revisionist arguments (despite their promise in the Preface not to revive the debate between revisionists and their critics) and more precision in explaining where disaggregation of economic, diplomatic, and security factors is, and is not, possible. The editors only complicated the authors' difficult task of assigning relative weights to economic, diplomatic, and security influences. The final chapter (1961-1980s) is the worst on this dimension (too heavy on economic analysis and factors; too light on domestic and international politics), but several other chapters also leave the reader with little overall sense of what was most, less, and least important, except for general statements about "economic" and "noneconomic" factors underlying a particular event or period.

David A. Deese

*Boston College*

**Reason and Realpolitik: U.S. Foreign Policy and World Order.** By Louis Rene Beres. (Lexington, Mass.: D.C. Heath, 1984. Pp. 143. $20.00, cloth; $9.95, paper.)

If the United States as the major actor on the world stage wants peace, then it must prepare for peace.

In *Reason and Realpolitik*, Louis Rene Beres presents a passionately argued critique of American foreign policy, written with the sound and committed scholarship that characterized his earlier works on international politics and world order.

He warns that the world is walking on the edge of a precipice and that only a dramatic turnabout in our understanding of the current crisis and the implementation of appropriate policy changes can avert imminent disaster.

Beres begins by tracing the origins of realpolitik back to the 1648 Peace of Westphalia, which led to the formation and consolidation of centralized nation states in Western Europe. He decries the competitive power politics and balance of power strategies that failed historically to create international peace and that in today's world lead to self-destructive behavior. He maintains that outmoded ways of thinking and acting both underlie much of America's current search for a just and durable peace and dominate U.S. policy toward the USSR and the Third World. America therefore must begin to fashion its foreign policy on a new set of premises based on an "understanding that this nation coexists with others on this endangered planet in a perilously fragile network of relationships. Racked by insecurities, poverty, and inequality, this network can no longer abide the conflictual dynamics that have shaped international relations" for over three centuries (p. 2).

The central chapters of this disquieting book deal with the issues of nuclear weaponry and human rights. Beres holds that U.S. policies have led to the proliferation of the nuclear arms race and that the prevailing strategies could lead to omnicide. He joins the growing number of critics calling for a drastic rethinking of nuclear policies and is a strong advocate of nuclear arms control with the USSR.

The author contends that current administration policies on nuclear strategy and human rights are interrelated, and he decries the present U.S. tendency to overlook human rights violations in certain anti-Soviet states.

Although holding no brief for the Soviet Union, Beres is deeply disturbed by President Reagan's "evil empire" imagery. "Although Soviet behavior in world affairs hardly meets the test of 'goodness,' the alleged contrasts between the forces of evil and the forces of good represents little more than a childlike caricature" (p. 127).

Beres calls for a new definition of national interest, one that moves away from the long-standing adherence to Social Darwinism in world affairs. He calls upon the United States to help develop a more harmonious system of planetary political life—to seize the initiative for our common survival while there is still time. If it did so, "the U.S. could confront the dying world of apocalyptic militarism with wisdom and hope."

In his concluding chapter, "From Paradigm to Planetization: The Journey Begun," Beres agrees with Thomas Kuhn regarding the need for transformation of U.S. foreign policy behavior from old forms of power politics to a world order orientation. Before the behavior can change, there must be a widespread "revolution of consciousness." An end to realpolitik requires an expanded awareness of global interdependence. Beres cites teachers over the centuries, from Marcus Aurelius to Teilhard de Chardin, who are in the tradition of this wholistic mode of thought.

Although Beres is wise enough to refrain from trying to lay out a detailed blueprint for change, he makes a number of pertinent general suggestions. Calling upon Americans to return to the Enlightenment principles on which the nation was founded, he urges that militaristic nationalism be replaced by a recognition of the right of self-determination of all peoples, as codified in the U.N. Charter.

Although occasionally a bit turgid in its prose, *Reason and Realpolitik* is a most insightful and challenging book. It is both a manifesto for human dignity and a plea that the "business as usual" attitude with which we often study international relations be replaced by an incisive urgency. It can be recommended as "must reading" for policymakers, teachers, and students of international politics.

ARTHUR STEIN

*University of Rhode Island*

**Multidisciplinary Perspectives on Population and Conflict.** Edited by Nazli Choucri. (Syracuse, N.Y.: Syracuse University Press, 1984. Pp. xi + 220. $30.00, cloth; $13.95, paper.)

This is a noble attempt to make explicit some of the most enduring, but rarely confronted, assumptions about the relationship between population variables and conflict dynamics. Nazli Choucri heads a list of senior academicians from a number of disciplines in the social sciences in an attempt to sensitize us to the "potential political effects of population factors and their possible impact on conflict behavior . . . [and] to place demographic variables in a broader socioeconomic and political context and show ways in which they interrelate" (p. 1).

In a promising introductory essay, Choucri relies heavily on her previously published work to establish, although never quite unequivocally, that although population size is not a critical factor itself, other intervening variables often make demographic variables salient. She then proceeds to argue that four attributes of population—size, change, distribution, and composition—play a particularly significant role in international disputes, especially when they operate in concert. However, instead of demonstrating how this relationship might work in reality—a task that Davis B. Bobrow accomplishes rather successfully in a subsequent chapter—Choucri relies on lists of "crisis factors," "linkage factors," and "conditions for international conflict."

Although the introductory chapter leaves one with the distinct impression that the book will be about the place of population variables in *international* conflict, the volume's next three chapters focus almost exclusively on internal societal conflict. In fact, this is one of two ambiguities that seem to permeate the entire volume and that I find confusing and frustrating; the other is the intended audience of the work. It is clearly not appropriate for class adoption; nor does it report on new research or advance new theories; nor is it likely to be of much use to specialists either in the area of population or conflict; nor, finally, is it directed to policymakers at either national or international level. In fact, the policy lessons made explicit in almost all chapters are at best elementary with two exceptions: Bobrow's lucid synopsis and Robert C. North's learned overview.

The volume never quite manages to come to terms with the complexity of the relationship among population growth, development, and internal and/or international conflict. In fact, I feel that, in terms of the explicitly stated central theme of the book, the individual essays do not take one much further than the 10 propositions summarizing theory and evidence on population and conflict that are succinctly stated in the Introduction (p. 15).

One is also left puzzled by the many issues that belong in a book on population and conflict but that were ignored. Surely, the questions of internal and international migration and forced population movements would be natural candidates for a book on population and conflict. Furthermore, the role of the state in population matters—both in its ability to influence demographic variables and in its capacity to influence internal and international conflict directly and indirectly—might have received considerably more attention than it does. Finally, why do the essays by Harold M. Proshansky, William R. Kelly and Omer R. Galle, and John R. Harris and Vijaya Samaraweera fail to come to terms with the reality that this was, or should have been, a volume about addressing population-induced conflict in types of societies where the issue is most acute: the less-developed countries.

In spite of a strong introductory chapter, the volume never delivers on its promise. An opportunity to address an important topic involving a putative relationship of enormous significance (see the World Bank's *World Development Report,* Oxford University Press, 1984, especially Part 3) has been lost.

DEMETRIOS G. PAPADEMETRIOU

*Population Associates International*

**The Problems of Plenty: Energy Policy and International Politics.** By Peter F. Cowhey. (Berkeley: University of California Press, 1984. Pp. xiii + 422. $36.00.)

This study is an impressive piece of scholarship on a very difficult subject. It captures the complexity of world oil markets through a carefully considered set of concepts that enrich our understanding of energy as such and improve our general understanding of the way that politics and markets intersect. Students of international politics and political economy would profit from reading this work, because its approach is applicable to other substantive areas.

Cowhey casts his analysis at the level of strategic interactions of private firms, government agencies, and international organizations. These actors design self-interested strategies of competition and cooperation within a structured and politicized market that shapes their choice of actions.

*The Problems of Policy* criticizes regime analyses that try to explain major international outcomes in terms of the power of the world's leading country or hegemone. Cowhey nicely pinpoints the political reductionism of this perspective that places undue weight on the political and the structural to the detriment of the dynamic process of constant mutual adjustment that is the hallmark of markets.

Cowhey also takes on neoclassical economic assumptions about the primacy of efficiency and automatic adjustments of profit-maximizing firms in markets. Instead, he finds that actual oil market outcomes vary from what the economic model would predict. How should we account for the variance?

The author explains these outcomes, unanticipated by the pure economic or the pure political models, by showing us how politics shapes market structure (the number of firms and their market shares) and the way market structures mold the subsequent choices—or "International Management Strategies"—of commercial firms and states acting as individuals and within coalitions. The mental image here of constant mutual adjustment among public and private actors is quite different from the more purposeful, directed, and dominant actor perspective that we associate with international regimes.

The core of this argument of strategic calculation within a coalition hinges on Cowhey's identification of two key aspects of this process. One is the degree of intervention into markets; the second is the degree of flexibility in implementing interventions. The former can range from detailed accords that directly allocate resources (such as the Red Line Agreement), to less interventionist means that set operational standards, to merely setting targets. A coalition's coordinating style in turn may involve strict "rules," more flexible "plans," and still more flexible agreements of "mutual adjustment." Cowhey applies these categories to show how influential governments seek to use firms as their surrogate managers of world markets (p. 79), even as firms press governments to control market rules and outcomes to private advantage. This approach allows Cowhey to avoid the false dichotomy between pure cartel and pure competition. In reality, there are elements of each in different activities at different times in different places. Three examples are offered as a loose "test" of his theory.

But the richness and value of the book lie not only in its theoretical ambition and its elaboration of concepts of corporate and political strategy and the conditions under which different mixes of strategy will be employed. The book also provides a useful description of key events and processes in the history of world energy markets—oil, nuclear, and coal. For example, Cowhey reminds us that the principal market management dilemmas have historically been managing energy surpluses, not shortages.

As with all works, its strengths are its weaknesses. A net cast wide enough to capture the many details and dimensions of oil market complexity runs the risk of overwhelming the reader with an escalating number of analytic categories. At several points Cowhey asks the reader to negotiate a seemingly endless string of strategies, goals, targets, rationales, or resources that together have the effect of diluting the argument's central thrust—the relationship between coalitional structure and choice. Cowhey recognizes this danger and provides charts and useful summaries along the way. These summaries are helpful and partly overcome the limitations of the category proliferation.

Despite these limitations, *The Problems of Plenty* is probably the most successful single work on the world oil market that tries to combine an unusually wide and largely realized theoretical reach with historical and empirical richness.

ERNEST J. WILSON, III

*University of Michigan*

**International Law and the Superpowers: Normative Order in a Divided World.** By Isaak I. Dore. (New Brunswick, N.J.: Rutgers University Press, 1984. Pp. xiv + 202. $30.00.)

Despite its slim size, this study is an important and ambitious work. As Dore states, it aims "to catalog in detail, norm by norm, the substantive content of the autonomous regime operating at the 'bloc' or hemispheric level and to identify its unique characteristics" (p. xiii). In substantial measure, it achieves this end, and in the process, furnishes a better understanding of the operation of superpower bloc politics in general and of the evolution of relevant international normative considerations in particular.

Dore appropriately begins his study with a treatment of where and how international law fits into the scheme of Marxist-Leninist ideology, with a view to explaining the significance of peaceful coexistence as a guiding concept for socialist legal thinking in the international arena. The thrust of his analysis is directed at applying a situational, case study approach (or as he terms it, a "behavioral approach") to assess the superpowers' conduct in crisis management, particularly within their own respective regional blocs.

Somewhat unrelatedly, the latter part of this work treats arms control efforts made by the superpowers in demilitarizing the ocean floor and outer space.

The study's main contribution is to be found in the chapters dealing with the superpowers' intrabloc behavior. Specifically, here Dore seeks to investigate two fundamental questions: (1) Are the superpowers' extraterritorial interventions merely acts of lawlessness in a normatively anarchical international society earmarked by ideological rivalry? Or, (2) Do these occasional interventions reflect something more rational about the dynamics of superpower tensions and interbloc relations? To address these questions, Dore first evaluates the legal conceptualization of aggression within both contexts of socialist and Western perceptions of the just war doctrine. He then applies these perceptions of "aggression" to intrabloc "trouble-situations" experienced by each superpower since World War II (i.e., for the United States, the cases of Korea (1950), Guatemala (1954), Cuba (1962) and the Dominican Republic (1965); for the Soviet Union, Hungary (1956) and Czechoslovakia (1968). Importantly, Dore avoids the polemics of adjudging the precise legality or illegality of specific interventions. Instead, he concentrates on how either United States or Soviet representatives employed international law to bolster their respective legal defenses of action.

Based upon these limited case studies, Dore's findings and conclusions are significant indeed, as they indicate strong evidence that certain rules (or an "emerging law of interbloc reciprocity") have grown up affecting superpower conduct. Among the more notable of these "laws" are that: (1) mutual noninterference should govern superpower relations in bloc affairs; (2) a breach of bloc solidarity remains a matter solely for bloc determination; (3) regional bloc preservation takes precedence over U.N. Charter provisions; (4) bloc members have rights of individual and collective self-defense against "indirect" aggression (read to mean subversion) in order to preserve ideological purity in the region; and (5) members of a bloc cannot withdraw voluntarily from bloc jurisdiction. To be sure, the study can be faulted for drawing such broad-based and far-reaching conclusions from so small a number of case samples. Yet, the policy patterns and conduct of the superpowers strongly suggest that these rules have been operative in their relationship during the past three decades, hence aptly denoting the realization that mutual respect for bloc solidarity has become a norm of superpower existence. This observation prompts Dore to make a more provocative general conclusion, namely that ideological conflict, far from obviating legal norms and lawful behavior, "is in fact the *reason for* the emergence of law" (p. 94). Certain norms have evolved, not from superpower fiat or dictate, but rather from the political realities of conflictive existence. In the sense that law is seen as a series of prohibitions that establish and reinforce expectations of what should or should not occur, much truth may lie in Dore's normative characterization of intrabloc superpower conduct. Even so, in extrahemispheric interventions and adventures (e.g., Angola, Zaire, and Afghanistan), these rules tend to break down under the weight of power politics and ideological rivalry. That realization should not be lost in the search for a law of interbloc reciprocity predicated on status quo regional arrangements.

There is much to be learned from a careful reading of this work. Dore's style is lucid, cogent, and well articulated, and his arguments are amply documented to substantiate his contentions. Further, Dore supplies a reasoned, sober, calculated analysis of superpower politics and their normative implications. In doing so, he sheds light on the role of law in the Marxist construct of international society, as well as how overriding concern for bloc preservation by both the United States and the Soviet Union has worked historically to circumvent law when bloc solidarity appeared threatened. Perhaps most important, the behavioral approach employed in this study underscores the nature and substance of "the living law of power politics." Yet, while force often may represent the antithesis of legitimacy, the use of force in international relations does not necessarily imply that there is no law. That is the chief lesson to be gleaned from this work, and in an age dominated by such seemingly volatile ideological rivalry, it remains a lesson well worth remembering.

CHRISTOPHER C. JOYNER

*The George Washington University*

**Assumptions and Perceptions in Disarmament.** By Daniel Frei. (New York: United Nations Publications, 1984. Pp. xiii + 321. $14.00.)

The premise of this study is that subjective factors dominate the prospects for serious arms control and disarmament. The Soviet Union and the United States are found, by way of an extensive qualitative content analysis, to be locked into a pattern of "conflictive cognition." Frei concludes: "The two sets of fundamental assumptions define the room for maneuver available, in principle, in disarmament negotiations. As they are almost mutually exclusive, the potential left for a common understanding seems at first glance to be rather small" (p. 278).

There is much to be admired in this study. Frei examined a large volume of literature. He focused on recent writings. He did not let his hopes dictate his conclusions. Most importantly, he organized his research and presentation around a common checklist of cognitive dimensions, which added rigor to a method too easily and too often misused for polemic ends.

On the other hand, the study is not without problems. The description of the U.S. perspective distinctly exaggerates its hawkishness. Why did this happen? For one thing, the search for a viewpoint biases one to find one. In the context of the Reagan administration, executive pronouncements become the operational definition of U.S. views. Granting executive branch dominance of national security affairs, nevertheless, nonexecutive institutions, especially the Congress, are important parts of the policymaking system. And the views of many of those actors are decidedly less hard-line than the administration's. U.S. hawkishness also is exaggerated by the sampling of writings. Official documents are supplemented by articles and books by such past officials as Kissinger, Brzezinski, and Harold Brown, but not from Vance and Warnke. The review of the academic literature seems, too, to be less than systematic and hence overly hard-line in the composite picture portrayed.

The description of the USSR seems also to be overdrawn. Frei presumes a unity of views: "The specific origin of all officially published material in the Soviet Union also suggests that looking for discrepancies reflecting latent tensions between different 'wings' may probably be nothing but a futile task. Western observers sometimes try to identify different and conflicting perspectives. . . . However, there are compelling reasons to assume that interpretations of this kind are ultimately based on wishful thinking and may be simple projections of the pluralist Western approach on to Soviet reality" (p. 26). This presumption can be just as blinding as the myopia he criticizes.

The degree of pluralism or uniformity of Soviet views is, ultimately, an empirical issue; it may vary according to the generality of views. Fundamental axioms, which are Frei's main concern, may very well be adhered to with considerable homogeneity. Their application to specific problems may give rise to the considerable debate across Soviet schools of thought and interested organizations.

If this is correct, then Frei has clearly shown that a general halt to the military competition is virtually impossible. He has less clearly shown the inevitable frustration of moderate arms control efforts.

These quibbles notwithstanding, Frei's analysis seems fundamentally sound. This sobering book is a useful, even important contribution to the arms control literature.

Patrick Callahan

*DePaul University*

**The Soviet Union and Terrorism.** By Roberta Goren. Edited by Jillian Becker. (Winchester, Mass.: Allen & Unwin, 1984. Pp. xii + 232. $25.00, cloth; $11.50, paper.)

At a time when senior policymakers have expressed their concern over the threats to U.S. interests overseas posed by "state-sponsored terrorism," Roberta Goren has written a work that places a major element of that threat into a rich historical perspective and current analysis.

In *The Soviet Union and Terrorism,* she has provided both policymakers and academics interested in the topic with a valuable book. It is regretable that the book was written as an academic thesis that was completed only a few weeks before the author died, because based on her initial research, it is obvious that Goren had a great deal more to contribute to the field. However the work is a fine and enduring contribution to the literature on terrorism.

The book is essentially an excellently researched descriptive study of how the Soviet Union has systematically employed terrorism as a weapon of political warfare and armed conflict since the time of Lenin.

After presenting the two categories of terrorism to be studied—subrevolutionary and revolutionary—and effectively dealing with the problem of definition in the Introduction, Goren makes a trenchant case in chapter 2, "Marxist-Leninist Ideology on Terrorism," for her view that the ideology is far more than "mere rhetoric" used to justify the use of terrorism; that it provides the motivation, organization, and strategy that makes terrorism an offensive weapon in Moscow's conduct of its international affairs.

Chapter 3, "Legal Aspects," is particularly germane to an appreciation of how the USSR has developed the capacity to sponsor various terrorist groups. By defining and making operational the concept of "indirect aggression" to meet its own goals, the Soviet Union increasingly engages in "surrogate warfare [that] has . . . increasingly become an alternative mode of conflict" to conventional military confrontations (p. 40).

Chapter 4, "Historical Perspectives," provides a detailed analysis of how the Soviet Union has pragmatically employed terrorism in conjunction with traditional diplomacy in achieving foreign policy objectives. She notes that even when an emphasis was placed on the use of diplomacy as a result of the United Front policy against facism in the 1930s, "this new emphasis . . . did not herald any reversal in the use of terrorism. International terrorism was in fact never abandoned; it simply receded into deeper secrecy" (p. 64). Furthermore, Goren correctly observes that despite an ideological "abhorrence" against the use of "individual terrorism," there was no reluctance to resort to employment of "organized and centrally controlled terror methods and groups" (p. 87).

How the Soviet Union developed the ability to utilize highly diverse terrorist groups in the conduct of its foreign affairs is the subject of chapter 5, "Contemporary International Terrorism." Goren's study of the complex development of the relationship between the PLO and Moscow is particularly penetrating because it provides a basis to appreciate how "the links of the PLO with all major terrorist groups operating in Western liberal democracies, and some Third World countries, have allowed the Soviet Union a means of having global impact" (p. 139). These linkages are discussed in a very readable account of major terrorist groups that is reminescent of Claire Sterling's fine treatment of the topic in *The Terror Network* (Holt, Rinehart & Winston, 1981).

In the final analysis, this book has substantiated and illuminated the first sentence in the Introduction. "This book is about war: the war which the Soviet Union is waging on the West—indirectly. In practical terms, international terrorism is power projection. It is the physical manifestation of a bid for power" (p. 1).

One can only regret that we will no longer have the opportunity to share the insights of a promising scholar.

STEPHEN SLOAN

*Air University; University of Oklahoma*

**China's Foreign Relations in the 1980s.** Edited by Harry Harding. (New Haven, Conn.: Yale University Press, 1984. Pp. xv + 240. $18.50.)

**China and the World: Chinese Foreign Policy in the Post-Mao Era.** Edited by Samuel S. Kim. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 354. $42.50, cloth; $16.95, paper.)

Although both these books are edited collections dealing with Chinese foreign policy, they are different in intention and substance. *China's Foreign Relations in the 1980s,* despite its title, is a collection of articles approaching post-1949 Chinese foreign policy from several perspectives. Although there is no common argument to integrate the essays, the differing perspectives highlight the significant issues in studying Chinese foreign policy and compel the reader to reach his or her own conclusions.

The introductory chapter by Michael Hunt observes that there have been multiple foreign policy traditions during China's long history—ranging from the Middle Kingdom approach to participation in an amoral interstate system characterized by "Machiavellian cunning." History has left China with a security concern for peripheral areas and the search for a response to the humiliation by Western countries. Alternative responses have been "cosmopolitan" or "populist," entailing "dependency" or "autonomy." Although Hunt argues that Mao reduced tensions by combining strategies, Kenneth Lieberthal, addressing the role of domestic politics in Chinese foreign policy, reveals continued tensions between three contending policy packages. Lieberthal reveals that different foreign policies affect leadership stability and that leaders' domestic goals have encouraged particular policies. He argues that Mao promoted the Sino-Soviet dispute to enhance his power base, suggesting that the strategic triangle resulted primarily from domestic forces.

Bruce Reynolds examines Chinese international economic policy, distinguishing the factors shaping policy, including the restraints of the domestic economy and the international system. Of these factors—economic efficiency, ideology, and political strategy—the last has been most influential. The key of Chinese development lies in the reform program; if implemented, China will be able to take advantage of the opportunities in the international economy.

Steven Levine addresses a recurring issue in international politics: Are conflicts regional or global, and does the state's response accurately reflect the real situation. He argues that although China is a regional power and the issues it faces in Asia are regional, it has treated them as global, that is, as Sino-Soviet issues. China thus needlessly exacerbates conflict, particularly concerning Vietnam's role in Indochina. Yet insofar as Sino-Soviet conflict is primarily waged in Asia, regional and global may be identical in this case. Jonathan Pollack analyzes China's global role, emphasizing the international restraints on policy toward the superpowers. The strategic triangle emerged when China played balance of power politics to offset the Soviet threat. In contrast to Levine, Pollack asks what accounts for China's crucial role in international politics despite its relative weakness. Pollack's approach also contrasts with Lieberthal's emphasis on domestic politics in foreign policymaking.

Harding concludes the volume with an overview of China's changing role in international politics since 1949, periodizing the history into five eras and observing the changes and continuities in Chinese policy. He argues that in contrast to past cyclical movement between extremes, the current independent, moderate policy will be relatively stable. Together, these essays provide much intellectual stimulation for students and scholars of Chinese foreign policy, and are useful for those seeking a general understanding of Chinese policy.

*China and the World* has more ambitious goals. It tries to approach Chinese foreign policy within the context of political science theory, bringing together the work of China scholars and specialists in international politics. The case studies, however, for the most part fail to incorporate the theoretical or methodological approaches called for in the first part of the book. This weakness underscores the necessity for further work of this kind.

The introductory essay by Samuel Kim delineates the various approaches to Chinese foreign policy and points the direction for fruitful research by disaggregating the various aspects of the study of Chinese foreign policy. Essays by Davis B. Bobrow and Steve Chan and by Michael Ng-Quinn argue for methodological rigor and systemic analysis. Ng-Quinn argues that the world is bipolar and that Chinese policy should be viewed from this perspective. Unfortunately, he examines primarily the theoretical merits of bipolarity over multipolarity, giving little attention to actual Chinese policy. Susan Shirk's essay discusses domestic politics and foreign policy. She offers an excellent analysis of the regional, sectoral, and bureaucratic politics of foreign trade policy. Yet this is a unique issue area, and its implications cannot be generalized.

The second and third sections include many first-rate essays, equal in quality to those in *China's Foreign Relations in the 1980s*. For the most part, however, they do not deal with the issues raised in the first section. Steven Levine observes the limits to U.S.-China cooperation and analyzes various explanations for continued conflict. Chi Su explains improved Sino-Soviet relations in the 1980s from both Soviet and Chinese perspectives, and Donald Klein discusses policy toward the "Second World." Only Samuel Kim distinguishes between the reality and Beijing's own description of its third-world policy and between domestic and external sources of policy change, arguing that China's policy is more self-serving than its media indicates and that the source of its recent renewed interest in the Third World is primarily domestic politics.

The final section deals with various policy issues. Paul Godwin considers the fears of Beijing's defense establishment concerning Moscow and its resultant dissatisfaction with current defense spending and China's "independent" foreign policy. William Feeney discusses China's role in international financial organizations, emphasizing PRC impact on the organizations and China's use of them in its development plans. Dennis Fred Simon places China's quest for foreign science and technology in the context of overall PRC foreign policy, arguing for the continuity in policy, regardless of instability in bilateral relations. He also considers the various international and domestic obstacles to Chinese science and technology acquisition. Only Bruce Cummings explicitly uses theory, arguing that the international capitalist system, with the United States as the hegemon, dictates PRC international economic policy. PRC policy has conformed to U.S. desires and is the result of U.S. pressure. Allen Whiting considers the implications of the various authors' conclusions for the discipline and uses the various analytical perspectives to consider future Chinese behavior on issues likely to be of continuing concern to the international community.

The essays in this volume are highly informative. Moreover, this is an important volume because it represents a significant step in integrating the study of Chinese foreign policy with the international politics discipline. It should be read by both graduate students and by scholars interested in Chinese foreign policy.

ROBERT S. ROSS

*Columbia University*

**The Illogic of American Nuclear Strategy. By** Robert Jervis. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 204. $19.95.)

Even though this book, like many other well-reasoned, logical presentations, may have little impact on America's headlong rush to acquire all kinds of irrelevant and dangerous nuclear weapons systems, it should be read by as wide an audience as possible in the forlorn hope that reason may yet prevail. Although Jervis is at times almost Thomistic in his method of argument with objections, replies to objections, and counter replies to replies, the book does make a great deal of sense and can be understood by the intelligent layperson willing to devote undivided attention to the argument. The subject matter is obviously crucial, and Jervis brings to it extensive knowledge in the area as well as impressive analytical rigor.

Jervis starts from the assumption that nuclear weapons have made traditional ways of viewing international relations and the use of force irrelevant. Unfortunately though, much of the thinking in this area still uses categories and methods of analysis that might have been appropriate to the world of von Clausewitz but are no longer so. The problem is graphically presented in the opening line, where Jervis says: "A rational strategy for the employment of nuclear weapons is a contradiction in terms" (p. 19). Never before in history has the potential loser of a war had the capability to totally destroy the potential victor. In other words, nuclear weapons offer no defense, no matter how much either side spends on weapons.

The tensions and inconsistencies generated by weapons that do not fit into previous ways of viewing things have led to a variety of what Jervis calls "escapes." Briefly dismissing three of these escapes—defense, total nuclear disarmament, and the nuclear freeze—the author then focuses on "conventionalization," which underpins much of current American nuclear doctrine. This view regards nuclear weapons as if they were essentially like, but simply bigger than, conventional ones. Jervis devotes one critical chapter to a major implication of conventionalization: the argument that American security requires the establishment of escalation dominance, or the ability to defeat Soviet aggression at all levels of violence short of all-out war.

Having presented some of the problems and inconsistent assumptions prevalent in much strategic thinking Jervis then devotes two chapters to an analysis and critique of the present countervailing strategy which builds those fallacious assumptions into full-fledged incoherence and contradiction. Although some early indications of the policy can be found in the 1950s and 1960s, it is usually dated from the late 1970s under Secretary of Defense Harold Brown and has been continued with only cosmetic modifications by the Reagan administration. Although these chapters require close reading, they are worth the effort because of the vigorous way in which they demolish Brown's argument.

Having demonstrated that countervailing strategy is not necessary for deterrence, Jervis concludes by showing that the dangers and costs of the strategy outweigh any supposed gain that it might have. These costs include the expense of the weapons required, the strain on the European alliance, the fact that the strategy might be perceived by the Russians as a threat to their security, and the possibility that decision makers might begin to believe the fallacious assumptions of the strategy leading them to suppose that they could control high levels of conventional violence and even limited nuclear wars. In addition, more general political problems might result from this strategy including the tendency to believe ourselves that we must deploy the necessary forces to be able effectively to deter and then—when the forces are not forthcoming—believing that we do not have effective deterrence capability when in fact we do. Finally, there is the problem that the Soviet Union might become so desperate because of internal and external pressures as to perceive that war is necessary or inevitable. Although Jervis offers little in the way of alternatives, that is not his purpose. Pointing out the problems and dangers in current American nuclear policies is a sufficient accomplishment for any book, and this one does that so well that I would recommend it as must reading for everyone concerned with the fate of the world.

PHILIP W. DYER

*University of Nebraska-Lincoln*

**The Dynamics of Latin American Foreign Policies: Challenges for the 1980s.** Edited by Jennie K. Lincoln and Elizabeth G. Ferris. (Boulder, Colo.: Westview Press, 1984. Pp. xvi + 325. $35.00, cloth; $14.95, paper.)

As the editors of this volume suggest, scholars have generally treated Latin American countries as an object of U.S. foreign policy attention rather than as a set of autonomous or semiautonomous national units capable of formulating and implementing foreign policies of their own. This is partly a reflection of historical reality in that Latin America's economic dependence created a set of structural constraints for national policy makers that encouraged reactive behavior rather than active initiatives taken within a coherent foreign policy framework. The editors further suggest that when the foreign policies of individual Latin American countries have been studied, little use has been made of the comparative theoretical perspectives available in the broader foreign policy literature. The case study method has predominated, with scholars at best analyzing the impact of select national attributes such as capabilities, level of national development, and ideology.

Lincoln and Ferris attempt to move regional foreign policy analysis beyond these narrow confines by examining changes in the international systemic context that have encouraged the formation of more active and coherent regional policies and by presenting case studies to document these changes. Changes in the international system including increased multipolarity after World War II are viewed as having encouraged new adaptive foreign policy strategies aimed at maximizing national autonomy. A central focus of their analysis is the question of whether this movement toward greater autonomy will extend through the 1980s in light of the deepening regional economic crisis and increased U.S. propensity to intervene (e.g., the Falklands/Malvinas, Grenada, Central America).

The body of this book consists of a series of essays that further specify the nature of the regional systemic context and examine the foreign policy behavior of individual countries. Jack Child provides a thought-provoking examination of emerging conflict patterns in the region, arguing that the early post-World War II period of Latin American interstate relations was unusually tranquil. He sees it as being rapidly replaced by a more difficult period of ideologically motivated conflict in the Caribbean Basin and geostrategically motivated middle-power conflict in South America. Yale Ferguson traces the sporadic and perhaps reversible movement toward regional integration, and Paul Sigmund examines changing patterns of U.S. behavior toward Latin America.

Of the 10 essays that deal with individual countries, the most valuable in light of the book's stated purposes are those on Argentina, Brazil, Bolivia, and Mexico. Dennis Gordon convincingly argues that Argentina's post-Malvinas political and economic problems will result in a limited regional role through the 1980s. Wayne Selcher notes the increasing institutionalization of a broad-based and complex foreign policy community in Brazil and suggests that this community may resist central control and direction. Waltraud Queiser Morales places Bolivian foreign policy decisionmaking within the context of a regionally fragmented polity wracked by political instability. And co-editor Elizabeth Ferris notes six internally contradictory facets of Mexican foreign policy that explain external behavior as primarily a function of a deepening domestic political crisis.

Of the two general tasks implied in this volume's title (explaining the dynamics of area foreign policies and specifying the nature of these policies in the 1980s), the editors and chapter authors more successfully accomplish the latter. In spite of the inclusion of an excellent final essay by Ferris which suggests some hypotheses that might move us toward a truly comparative theory of Latin American foreign policies, the chapters are not written with such a set of hypotheses in mind. Rather, they represent some of the best that the traditional case study method is capable of producing. However, while we await the more theoretically interesting book that Ferris's concluding essay suggests will someday be written, we can benefit from a close reading of this one.

STEVE C. ROPP

*The University of Wyoming*

**The British Nuclear Deterrent.** By Peter Malone. (London: Croom Helm Ltd., 1984. Pp. 200. $27.50.)

The quixotic history of Britain's independent nuclear deterrent, and particularly of U.S. involvement with those forces, is nicely captured by two quotes. The first, from 1962, is Robert McNamara stressing the then widely held view of the American government that national nuclear forces are "dangerous, expensive, prone to obsolescence and lacking in credibility as a deterrent." By contrast, following discussions between President Carter and Prime Minister Thatcher in 1979 on British acquisition of the Trident SLBM that would maintain the U.K. independent deterrent well into the twenty-first century, the two leaders issued a joint communique in which they "agreed on the importance of maintaining a credible British strategic deterrent force and U.S.-U.K. strategic cooperation."

Granted, the U.S.-U.K. special relationship has not been the dominant factor in Britain's acquisition and modernization of an independent deterrent. As Peter Malone persuasively argues in this excellent analysis, the dominant feature of British policy has been precisely to avoid sacrificing "operational independence upon the altar of Anglo-American cooperation." Thus, despite the very tangible economic and technical benefits of the American connection, as well as the more symbolic political benefits of having an "independent" deterrent, British nuclear weapons policies ultimately have revolved around fundamental security considerations.

The history of these evolving strategic issues, beginning in 1940 when Britain became the first country to explore the military potential of atomic power, has been well documented before. Nonetheless, Malone brings to the subject both a comprehensive analysis, covering the relevant technical, political, economic and doctrinal issues, and a lively narrative style.

Where the book could be most interesting to those familiar with the topic is Malone's analysis of the current relationship of U.K. nuclear forces to Britain's overall strategic goals, and the conventional forces underlying those goals.

Interestingly, the debate now going on in Britain over the merits of a "maritime" versus a "continental" strategy is similar to that occurring in the United States. For the U.K., of course, even the appearance of such a debate is a marked departure from a centuries-old tradition of maintaining naval supremacy. Despite the decline of British resources in the postwar period and the retrenchment of Britain's military commitments, symbolized by the 1968 "east of Suez" withdrawal, the Royal Navy remained preeminent among the military services. As Malone points out, Minister of Defence John Nott was "dismayed" to learn in 1981 that more money was being spent on fleet air defense than on air defenses for Britain itself.

Yet the fact that the navy was forced to bear the brunt (57%) of defense spending cuts in 1981 proved only a temporary setback. As if in answer to one naval officer's supplication that "what we need now and quickly is a small colonial war requiring a lot of ships," the Falkland Islands crisis erupted in April, 1982. Following the Royal Navy's excellent performance in the south Atlantic, a December, 1982 review of defense priorities reemphasized capabilities for intervention missions, not only for the navy, but also for the army and air force.

Central to the debate on whether Britain can afford to shoulder both a continental option (British Army on the Rhine) and a maritime/quick response intervention capability is the expense of acquiring Trident SLBMs. For its part, the Thatcher government has argued that deterrence is better served by maintaining U.K. nuclear forces than by buying the two armored divisions and 300 tanks that cancellation of Trident could provide. As Malone notes, however, the fate of Trident should the Labour party return to power will not hinge on opportunity cost considerations alone. Strategic considerations of how Britain's military power can be maximized in a period of constrained governmental budgets, escalating costs of conventional weaponry, and antinuclear political sentiments will continue to foster a lively debate on the whys and wherefors of the U.K. independent deterrent.

JEFFREY BOUTWELL

*American Academy of Arts and Sciences*

**International Studies and Academic Enterprise: A Chapter in the Enclosure of American Learning.** By Robert A. McCaughey. (New York: Columbia University Press, 1984. Pp. xviii + 301. $28.00.)

This is a stimulating and revealing social history of an important sphere of American intellectual life. What Robert A. McCaughey has written about international studies may be somewhat discomforting for many who are associated with the field. But his thoughtful, if critical, scrutiny may also provide the basis for fruitful discourse over appropriate adjustments and changes that can maintain international studies as a thriving concern.

Part 1, "In the Land of the Blind" (1810 to 1940), traces the origins of American international studies as an "intellectual enterprise" and its gradual emergence as an "academic enterprise." Complementing the account are insightful career profiles of some of the individuals who played major roles in the initial period of this process (Archibald G. Coolidge and James H. Breasted, among others). McCaughey skillfully delineates the process by which international studies changed from being the realm of "gentlemen and scholars" to the "enclosed" preserve of academic specialists. It was a process that had its costs, along with the undeniable advantages. When intellectual pursuits came to be seen as primarily the business of universities, and recognition and status was conferred upon research-oriented professors, the full enclosure of international studies as an academic enterprise was inexorably brought to completion.

Part 2, "The Years that Were Fat" (1941 to 1966), considers the phenomenal transformation of the established academic enterprise resulting from the impact of World War II, the funding operations of the Ford Foundation, and the enormous expansion of American universities. The war and the immediate years afterwards brought the enterprise of international studies to its "takeoff" stage. Of seminal importance was the appearance of the Gaither Report, with its pervasive internationalism. Now international studies as an enterprise was truly on its way. The very magnitude of the Ford Foundation's funds—McCaughey discusses the foundation's struggle to match grants with its staggering income—required rapid expansion. Ultimately, however, its support concentrated on the universities with established reputations in international studies, while international studies academics were allowed to determine what areas to support. The results were not altogether laudable. McCaughey finds relatively little progress after 15 years of the International Training and Research Program (ITR) and about $250 million.

The analysis reveals a gap between needs and capacity. The massive intervention into the life of the enterprise hastened its full enclosure within the universities, thereby contradicting the explicit purpose that had prompted that intervention in the first place. The greatly reduced dependence on outside sources lessened the need to write for popular audiences. Indeed, popularization became "inappropriate." An epilogue addresses the so-called "crisis" in international studies following the termination of ITR. The nicely executed tallies of Ph.D.'s reveal some interesting information, not the least of which is that the end of ITR may have been in some respects a blessing in disguise. The data suggest, for example, a growing dispersion of production capacity and a leveling of institutional reputations after 1966.

McCaughey concludes that international studies is a "saturated activity." If, as an academic enterprise, it is beset by a "crisis," it might be mindful again of its earlier constituencies. That, however, would oblige the enterprise to be a little less self-centered. Hopefully, this well-written and illuminating study serves as a spark for serious reassessment. It is a work deserving a readership far beyond that of the international studies community, for it entails much that is of interest for anyone concerned about the future of higher education.

MANFRED GROTE

*Purdue University Calumet*

**Foreign Policy and the Modern World-System.** Edited by Pat McGowan and Charles W. Kegley, Jr. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 287. $25.00, cloth; $12.50, paper.)

One of the most exciting developments in recent international politics research has been the "rediscovery" of history. Spurred first by Immanuel Wallerstein's "capitalist world-system" model and later by George Modelski's "long cycles of political leadership" approach, many scholars are now reconsidering their research agendas and approaches. The essays in this book, Volume 8 of the *Sage International Yearbook of Foreign Policy Studies,* take these ideas and explore their relevance for the comparative study of foreign policy.

In their introduction, McGowan and Kegley assert that the comparative study of foreign policy has suffered because of an overabundance of narrowly focused, quantitative studies. They hope that consideration of a world-system perspective can result in "a more judicious balance between quantitative and qualitative, historical and current, case-study, comparative, and statistical research designs" (p. 9).

The first two essays focus on theoretical issues. After pointing out potential weaknesses in Wallerstein's model, James Lee Ray notes some of its potential advantages. Beyond increasing historical awareness, it offers more easily operationalized variables, greater parsimony in theorizing, and a potential way to integrate traditional international politics and the study of international political economy. William R. Thompson's essay directly compares the Wallerstein and Modelski models and notes their disagreement regarding "system time." For example, the contemporary period falls in different phases of the two models, producing different predictions and explanations. Like Ray, Thompson calls for a synthesis of the two models because Wallerstein's focuses primarily on economics and Modelski's on military and political issues.

Three essays are offered as historical studies. Michael S. Kimmel notes the domestic constraints faced by core powers in the sixteenth and seventeenth centuries. Harriet Friedmann addresses the interaction of economic logic and state power. She argues that a focus on price structures can marry the logic of economics to that of state roles. Michael Stohl examines the negative economic consequences of hegemony, which include a widening of income disparity and a weakened domestic economy.

Three particularly strong essays address the contemporary period. John Boli examines the paradox of Swedish foreign policy. Although a member of the capitalist core, Sweden often undertakes anti-core foreign policies. While noting the role of Olaf Palme and Sweden's history of neutralism, Boli says Sweden has the advantage of membership in the core without the financial or political responsibilities of supporting the capitalist world-system. Boli's emphasis on the role of Sweden as a subcore power makes possible generalizations to other subcore powers like Norway, Denmark, the Netherlands, Belgium, Switzerland, Austria, and Canada.

James Hawley attacks the reductionist element of Wallerstein's model, saying state actions are understandable only on the context of their times and are not economically determined. After studying recent American capital control programs, Hawley argues that the Wallerstein model ignores or underestimates the impact state actions have on the structure of the world-system and incorrectly assumes that state policymakers are responsive only to the dominant economic classes in their society.

In a frightening essay, Albert Bergesen asserts that we have passed through his first two phases of system change: the decline of the hegemonic state and the hegemonic state's reliance on treaties and alliances to preserve its power. Bergesen says we are entering the third phase of open friction and international incidents. The final phase is systemic war. Bergesen predicts that World War III will begin between the superpowers' third-world clients. If nuclear holocaust does not result, both the Americans and Soviets will be too economically weakened to be the postwar hegemonic power. Japan will assume this role based on its structural position, similar to that held by the United States in 1914, and its leading role in the merger of state and firm, which Bergesen sees as the economic transformation of the future.

Overall, two essays do not fit well with the others. Christopher Chase-Dunn offers an idealistic prescription for a socialist world-system, and Michael V. Esler presents a comparative foreign studies bibliography for the period from 1978 to 1981. The book suffers from the lack of an integrative conclusion, but the stimulation provided by the essays more than offsets these weaknesses. This work should be read by both researchers and graduate students.

RALPH G. CARTER

*Texas Christian University*

**Latin American Nations in World Politics.** Edited by Heraldo Muñoz and Joseph Tulchin. (Boulder, Colo.: Westview Press, 1984. Pp. ix + 278. $30.00, cloth; $12.95, paper.)

*Latin American Nations in World Politics* is a welcome addition to the nascent field of Latin American foreign policy study. It departs from previous works by taking preliminary steps toward greater theoretical clarity. The introductory chapter provides a set of theoretical perspectives marking differences between external and internal sources of foreign policy. Following is a series of chapters that place interesting country and regional case studies within the conceptual categories outlined at the beginning. The volume rightfully emphasizes the enormous diversity of foreign policy options available to Latin American states. For too long, it was presumed that variations were not present given Latin America's dependence on the United States. As the book demonstrates, countries (like Brazil) with strong economic ties to the United States have shown remarkable diplomatic and political independence from—indeed defiance of—their North American neighbor.

External variables, including power politics and geopolitics, are integrated into useful discussions of the Caribbean Basin and the Southern Cone. Two authors connect on the declining U.S. military presence in the Caribbean region under President Carter with the rising influence of mid-level powers like Mexico, Venezuela, and Cuba. National frontiers and territorial expansion became important issues in relations between Argentine and Chilean dictators. On one hand, authors point out the interaction between objective boundary disputes and the heightened sensitivity to such disputes that arise with a change from democratic to authoritarian regimes.

On the other hand, the authors show how regime change is not always critical, particularly where the diplomatic core retains autonomy from political leadership and maintains its historical continuity. An interesting contrast is drawn between the highly independent foreign ministry in Brazil, which formulated progressive policy despite a conservative regime, and the Chilean foreign ministry under General Pinochet, which became an ideological tool of a president who never trusted the career diplomats. Other internal factors discussed in this volume include the role of political parties, presidential styles, bureaucratic politics, and dominant national projects.

Comparative foreign policy analysis is an important field of inquiry, and this book makes a contribution in that area as well. Chapters on the Caribbean link foreign policy differences and similarities between countries to the nature of the party system (one party in the case of Mexico and Cuba, and two party in the case of Venezuela and Colombia) and the relation of each nation to the superpowers (whether it is strong as in the case of Cuba and Colombia, or weaker as in the example of Mexico and Venezuela).

Competing explanations are often offered for the same country and regime. Thus, the foreign policy of the military junta of Argentina is explained both by the dominance and the failure of its national project. This is hard to avoid in a volume of this sort where so many different variables are discussed by numerous scholars. Perhaps the addition of a concluding chapter would have been helpful, in which the editors tried to synthesize what had come before. What is needed is a multivariable approach that parcels out the independent *and* interactive contributions of a set of factors to the development of Latin American foreign policy. It is to the editors' credit that they are conscious of their own limitations, calling for future conceptual schemes that "are sufficiently broad to allow for multicausal explanations" (p. 16). The volume is a very nice contribution to the field because of its attention to theory, the breadth of its focus, its comparative emphasis, its sense of its own limitations, and its reflection on the need to go further in the direction of theoretical rigor.

DAVID PION-BERLIN

*The Ohio State University*

**Negotiating Peace: War Termination as a Bargaining Process.** By Paul R. Pillar. (Princeton, N.J.: Princeton University Press, 1983. Pp. 272. $25.00.)

*Negotiating Peace* is a skillful effort to employ the theory of bilateral bargaining to understand the terminal phase of wars from the initial agreements to beginning negotiations to final settlement. It is a book with many strengths and a few weaknesses. A central theme is that the size and shape of the bargaining set, in particular whether it is convex, will influence two crucial elements in negotiated conclusions to war. First, it is persuasively argued that non-convexities in the bargaining set will produce unstable one-sided dynamics in the process of reciprocal concession between negotiators. Second, these non-convexities will accentuate the effectiveness of military violence on the battlefield as a lever in the diplomatic negotiating process. For both phenomena, non-convexities lead toward extreme or corner solutions. Next, Pillar argues that relevant non-convexities occur when the object of the war, the prize for which it is fought, is indivisible. For these, there must be a winner and a loser, compromise is infeasible, and thus mixed interior solutions unattainable. On the other hand, the spoils of war may be partially or wholly divisible or even partitioned in such a way that the aggregate benefit is higher than if one side grabbed everything; in this case, the dynamics of concession should be reciprocal, cost-imposing military ventures should exercise less leverage over the outcome, and the most likely negotiated solution will be compromise "interior" solutions. Taking this idea to a limit, a salient solution to the bargaining problem may bear resemblance to joint public good consumption.

As one travels conceptually from the one extreme of total indivisibility to the other of total jointness of consumption, questions of measurement and interpersonal (internation) comparisons of gains from settlement become especially important in the middle ground between extremes. Disappointingly, Pillar has rather little to say on this question except for the apt and correct observation that the slope of the bargaining set is logically derivable from the comparative importance of outcomes as between opponents relative to the comparative importance of costs of disagreements (p. 155). And the almost exclusive concentration on the Nash solution to the bargaining problem is a further source of disappointment. The opportunities to threaten actual damage may differ between opponents so that asymmetries in threat deserve more treatment. Similarly, better development of the consequences of side payments, of the ability to bind oneself in advance to commit to actions one will later regret having to perform, and various other Schelling-type maneuvers would have been profitable.

The book nevertheless repays careful reading. A limited empirical section with casual regression analysis yields partial support for the Cournot-Nash theories on which the work is founded. A more special strength, however, is the impressive assemblage of annecdotal material on war terminations in the nineteenth century through the Korean, Algerian, and Vietnamese conflicts. This is impressive scholarship relating the histories of internal decision calculus and of battlefield developments to the course of negotiations, with comparisons drawn between the predictions of bargaining theory and the actual course of events.

MARTIN C. MCGUIRE

*University of Maryland;*
*Institute for Defense Analyses*

**Hanging Together: The Seven-Power Summits.** By Robert D. Putnam and Nicholas Bayne. (Cambridge, Mass.: Harvard University Press, 1984. Pp. x + 263. $17.50.)

The interface between international and domestic politics is perhaps nowhere clearer than in the annual seven-power summits bringing together the chiefs of government of the most powerful advanced industrial states. Initiated in 1975 at Rambouillet in France (Canada was not yet a participant), the summits were convened every year, Bonn being the venue for the latest occasion in May, 1985. Considering the enormous significance that these annual meetings of the leaders of the United States, Great Britain, France, West Germany, Italy, Japan, and Canada can have for the solution of international problems and for possible cooperation, the historical account, analysis, and evaluation of the summits as an evolving process in international politics provided by Putnam and Bayne fill an important need.

The book is especially valuable for international relations scholars because it seeks to apply theoretical concepts to the summit process. It focuses on the reasons for the institutionalization and bureaucratization of the summitry, examines the interplay between economic interdependence among the individual democracies and the exercise of national sovereignty, explores the problem of reconciling the hegemonic role played in varying degrees by the United States since 1945 and the need for the collective management of economic activity by the summit countries, and discusses the well-known awesome ability of the bureaucracies to stymie the wishes of political leaders in connection with summit objectives.

The book covers the summits from 1975 at Rambouillet to 1983 at Williamsburg. The authors consider the summits as useful exercises, although they give the Versailles meeting of 1982 a failing mark and the London summit of 1977 a minimal success grade. Although some observers are doubtful about the value of the annual exercise, the authors believe that if the summits were to cease, "it would seriously weaken the cohesion of the West" (p. 214). Moreover, the personal ties established through the summit process between the governmental leaders of the major powers of the West "can lubricate international relations" (p. 197).

Not surprisingly, the summits have changed since 1975. They began as general and relatively informal discussions but quickly became bureaucratized and assumed a high degree of instituionalization. With months of preparatory meetings, large delegations drawn mainly from the finance, economics, and foreign ministries became involved. In addition, a number of relevant IGOs participated in the preparations of the summits, most prominently the OECD, GATT, and IMF. The reason for the extensive involvement of these organizations and their bureaucracies was that certain very important themes cropped up in all summits: problems of macroeconomic policy coordination, international monetary issues, and international trade. This raises the question of whether the summits, by establishing a large planning staff under the guidance of so-called sherpas (named after the Himalayas guides), were not duplicating the functions of existing organizations. Although Putnam and Bayne concede that it is "necessary to hold back the growth of bureaucratic apparatus," the right framework must be created "for the leaders to exercise their unique capabilities—for integrating varied subjects, balancing domestic and external pressures, and exercising high-level political authority" (p. 215).

The summits have become media events. Although in 1981 the number of reporters at the Ottawa summit was estimated to be about 1,500, at the 1985 Bonn summit nearly 4,000 print and television journalists had been accredited by the West German government (*Time,* May 13, 1985, 21). This is an unwelcome development, because the media have tended to present the summit as a "kind of international jousting match, and the summiteer as a national paladin, vigorously (and usually victoriously) defending national interests against obtuse and even malevolent foreigners" (p. 176).

Putnam and Bayne have made a significant contribution to our knowledge of and theoretical insights into an important international development. Their book is well worth reading by international relations scholars, historians, and those generally interested in the problems of bureaucracy. For theory construction, the implications of *Hanging Together* may be far-reading and may stimulate further empirical research. What, for example, is the extent and effect of frequent transnational bureaucratic interaction on middle and upper levels of the summit governments and involved IGOs? What are the parameters of educational and socializational processes spawned by the summit procedures and events? In which way do the personal interactions between the leaders enhance their empathy regarding the concerns and culture of the people in the other summit countries? Would it be theoretically feasible to regard the series of summits, because of their institutional development, as an evolutionary international regime for the coordination of national policies and other activities?

WERNER J. FELD

*University of New Orleans*

**The Soviet Union and the Czechoslovak Army, 1948-1983: Uncertain Allegiance.** By Condoleezza Rice. (Princeton, N.J.: Princeton University Press, 1984. $37.50.)

Condoleezza Rice has written a first-rate book; one that not only fills a major gap in our knowledge of civil-military relations in a key country in Eastern Europe, but one that also provides useful insights into the impact of the Soviet factor on Czechoslovak civil-military relations.

In essence, Rice argues that there is a built-in tension in Czechoslovak civil-military relations as a result of the need of the armed forces to be loyal to two masters—the USSR and the Czechoslovak Communist party. As long as Moscow and Prague's interests coincided, this tension was manageable. When they began to diverge, however, problems developed. For example, Khrushchev's decision to permit greater emphasis on national identity led to a search for the armed forces' national traditions. The events surrounding the Prague Spring intensified this process and led to a push for rapprochement between an increasingly independent party leadership and a military that was looking for Czechoslovak solutions to the country's political-military problems. The Soviet invasion reversed the rapprochement process and in the crushing purge that followed in 1969, the armed forces were decimated. As a result, the tension of the sixties disappeared. But the cost was high. Not only is the military estranged from the populace (which sees it as a tool of the USSR), but the quality of officer and noncommissioned officer cadres also remains low.

Rice's attempt to take Czechoslovak civil-military relations out of their purely domestic context and place them within the broader perspective of Soviet-East European relations is to be welcomed and marks an important step forward in the analysis of civil-military relations in communist systems. It will take time and many more studies—as Rice acknowledges—before we will find the proper balance between the domestic and Soviet factors, but clearly Rice's book opens a fruitful path for further analysis.

Rice also provides a wealth of useful data on Czechoslovak civil-military relations. This is especially true of the 1968 events. She provides invaluable analysis of splits in the military (with the reformers coming primarily from the Main Political Administration), of the attempt of the senior military leadership to find a Czechoslovak way for dealing with the country's problems, and of the attempt to Sovietize the military in the aftermath of the Soviet invasion.

This book could have been improved by a more critical use of certain concepts. In speaking of Sovietization in the 1969 to 1970 period, for example, Rice focuses on three areas: doctrine, political education, and integrated (i.e., multinational) training. Although few will argue that the move against the development of a Czechoslovak military doctrine was anything but a clear sign of Sovietization, the other two indicators have much broader significance. The content and effectiveness of political education has as much importance as its frequency. Furthermore, despite the prominence that some writers have assigned to Soviet efforts to coordinate political education throughout the Warsaw Pact, its value should not be overestimated. It is noteworthy, for example, that many East European writers continue to lament its effectiveness. Likewise, the push for more integrated training has little to do with Sovietization. It is also a prerequisite for coalition warfare. It would be hard to imagine a combined arms operation without the necessary integrated training exercises beforehand. The book would also have been strengthened by more discussion of the Czechoslovak-Warsaw Pact connection, that is, What is the nature of the interface? Greater emphasis on the ethnic factor would have been useful (the appointment of Colonel General Milan Vaclavik, a Slovak, to succeed Martin Dzur as Defense Minister suggests that the ethnic factor remains a major concern in the military). Finally, the chapter on the period since 1975 strikes me as a bit superficial, especially when compared with the rest of the book.

Despite these criticisms, this book is a must for anyone interested in civil-military relations in communist systems. It is, as Timothy Colton has noted, "by far the best work yet written on military politics in contemporary Eastern Europe."

DALE R. HERSPRING

*Washington, D.C.*

**An International Law of Guerilla Warfare.** By Keith Suter. (New York: St. Martin's Press, 1984. Pp. x + 192. $27.50.)

Hans J. Morgenthau commented in a 1946 article in the *Yale Law Journal* that "the questions which the law and the lawyer answer are largely irrelevant to the fundamental issues upon which the peace and welfare of nations depend." Keith Suter has elaborated upon this notion in an excellent multilayered study of the 1977 creation of the two additional protocols to the Geneva Convention directed at the question of guerilla warfare. The book describes the "long journey" that began in the mid-1960s in the mind of Sean McBride, general secretary of the International Commission of Jurists, who sought to raise the issue of human rights in armed conflict. The journey continues with a resolution from the 1968 Geneva NGO Human Rights Conference and another from the 1968 Tehran International Conference on Human Rights requesting the Secretary General to initiate efforts to secure the application of humanitarian conventions in armed conflict and to assess the need for new conventions. The scene shifts to the United Nations at this point, where in 1968 the General Assembly passed Resolution 2444 (XXIII) affirming a review resulting in a series of reports from the Human Rights Division in 1969, 1970, 1971, and debates in the General Assembly. All of these efforts were to culminate in the 1974 Diplomatic Conference.

Suter is not content to merely chart the progress of the efforts to create the 1977 protocols. On the contrary, at each step of the way he analytically assesses the strengths and weaknesses of the various drafts and resolutions, arguing stoutly, for example, that the human rights approach to armed conflict confused a human rights regime and the laws of armed conflict. Further than that, he explores the variety of roles and pressures exerted by the NGOs, especially the International Committee of the Red Cross (ICRC). Suter notes that "the International Red Cross movement had the rare distinction of being simultaneously one of the best known international movements and one of the least known" (pp. 85-86). The Geneva Protocols of 1949 are the particular province of the

ICRC as they monitor observance of the provisions and periodically make efforts to update them. The ICRC had made such efforts before but were particularly vigorous in 1970 not to let the initiative and control of the process of drafting new conventions slip from their grasp. Through conferences held in 1971 and 1972, the ICRC developed two draft protocols to the four Geneva Conventions. The Geneva Conventions, as Suter points out, were sacrosanct and not subject to revision.

The culmination of the initiatives of McBride, the United Nations, and the ICRC was the series of diplomatic conferences held in 1974, 1975, 1976, and 1977. True to patterns observed elsewhere in massive multilateral diplomacy, the first gathering was a political ritual of third-world condemnation of racism, imperialism, Zionism, and disputes over procedure and recognition of liberation movements. The latter, of course, was particularly contentious as it was in fact part of the substantive concern of the meeting. The conference almost broke down but reconvened in ensuing years and produced the two protocols. As of 1983, only 31 states had ratified Protocol 1; only 25, Protocol 2.

Suter concludes his study with an assessment of the new law and asks what went wrong. A 20-year effort to update the law of armed conflict to make it applicable to guerilla forces produced protocols that would have minimal impact in such conflicts. Suter elaborates a number of reasons for failure, such as the third-world ambivalence on such conflict in which they wish to help national liberation movements but certainly not within their own borders. Suter suggest that the ICRC's unwillingness to go to the foundations and reassess the Geneva Conventions despite their "near moribund" state resulted in grafting overly complex protocols to an overly complex set of Conventions that speak little to the dominant form of contemporary armed conflict. The conservatism of the legal specialists contributed as well. Suter notes: "the Protocols are too long, too complicated, too full of qualifications and esoteric legal language to be practical ways of regulating conflicts" (p. 182). The new protocols were directed at both international and "non-international" level and provided neither the clarity, nor more importantly the incentive, for either governments or guerillas to grasp the provisions and to obey them.

Suter's study of the protocols is a fascinating parallel to the Law of the Sea conferences which produced a contentious but much more substantive result. Armed conflict cuts closer to the security and survival of states, and the weakness of the protocols may only be that it is, as Stanley Hoffmann said, "a magnifying mirror that reflects faithfully and cruelly the essence and logic of international politics" (in L. Scheinman and D. Wilkinson [eds.], *International Law and Political Crisis,* Little, Brown, 1968, p. xvii).

RICHARD B. FINNEGAN

*Stonehill College*

**Dangerous Relations: The Soviet Union in World Politics, 1970-1982.** By Adam B. Ulam. (New York: Oxford University Press, 1983. Pp. vi + 325. $7.95, paper.)

This book is the long-awaited and fitting update of Ulam's *Expansion and Coexistence: Soviet Foreign Policy, 1917-1973* (Holt, Rinehart & Winston, 1974). According to Ulam's preface, the book under review deals with the rise and fall of detente, the problem of nuclear weapons in world politics, how Soviet domestic politics in the 1980s is liable to affect Soviet foreign policy, and what the West might do to arrest its decline as a powerful actor in world politics. The latter two issues are dealt with briefly in the final chapter, and the reader cannot help but be disappointed that Ulam does not elaborate on the remedies to decline beyond noting the need for European integration and Western economic-political cooperation. The great strength of the book lies in its analysis of misperception in U.S. foreign policy regarding Soviet activities and objectives in world politics and its masterful depiction of the West, and particularly the U.S., as seen through Soviet eyes. One of the themes running throughout the book is the great fear the Soviets have of a nuclear China and the various means (almost always oblique) the Soviets have used in an attempt to get the United States to cooperate in the nuclear containment of China. For instance, Ulam relates, at the signing of SALT I in 1972 in Moscow, Brezhnev hinted that the USSR might help the United States extricate itself from Vietnam in return for U.S. cooperation in restricting the "nuclear aspirations of Beijing" (p. 75). In the SALT II agreement, the Soviets succeeded in inserting a provision that would have prohibited a Sino-U.S. strategic military relationship, and in the preliminary discussions on SALT III alluded to the China problem. Ulam states that the China problem contributed to the arms race: Concern with China "was one of the main reasons why the USSR accelerated its strategic as well as conventional arms build-up in the mid-sixties" (pp. 44, 50).

The main reason for the Soviet military buildup, though, even beyond distrust of a democratic (and hence unpredictable and erratic) U.S. and a revisionist China, is the inherently expansionist nature of Soviet foreign policy. In *Expansion and*

*Coexistence* Ulam related a minister's advice to Catherine the Great that sheds light on Russian foreign policy motivations: "That which stops growing begins to rot." During the Soviet period, and during the period examined by the book under review, the Soviet leadership could point to successes in foreign policy as legitimizing socialism, even though domestically socialism has not alleviated social, economic, and ethnic problems. Ulam stresses that during the 1970s Soviet expansionism achieved a new momentum which, once started, led to Soviet actions that destroyed U.S.-Soviet detente, even when a more restrained posture may have yielded similar results for the Soviets. Ulam warns that even though a younger generation is coming to power in the Soviet Union, the momentum of expansion will be hard to brake. He also points out that members of the younger generation, instead of being less expansionist in foreign affairs, may see expansionist policies as natural, having spent their formative years witnessing the growing might of the USSR and the weakening of the West.

Ulam's analysis of the rise and fall of detente emerges as superbly crafted, well-written, and even humorous, based on examples of ludicrous Soviet propaganda and posturing and on American leaders' acceptance of ludicrous Soviet propaganda as reality. Ulam has an encyclopedic grasp of the events of the 1970s that makes his book seminal both to the student who is reading his or her first book on Soviet-U.S. relations and to the Sovietologist who will appreciate the broad overview of U.S.-Soviet relations.

JAMES P. NICHOL

*Library of Congress*

# Normative Theory

**The Rise and Fall of Structural Marxism: Althusser and His Influence.** By Ted Benton. (New York: St. Martin's Press, 1984. Pp. xv + 259. $27.95.)

In closing his vigorous interrogation of Louis Althusser, his followers, and his critics, Ted Benton writes, "Like many other socialists of my generation, my own ideas and political activities have been influenced over the past decade and a half by the ideas and arguments presented in this book, sometimes beneficially, sometimes in ways I now regret" (p. 227). This observation illustrates the need for Benton's accessible critique of "structural" Marxism and Benton's qualifications for executing it. Although Althusser and his influence have pervaded European debates in one way or another (in the case of Marxists to the point of being virtually second nature) American social theory has remained largely unaffected. Benton's "kind of settling of accounts with my own intellectual and political biography" (p. 227) enriches socialist praxis and can inform American social science.

Benton's contributions can be summarized by three adjectives: contextual, incisive, and engaged. First, Benton situates Althusser's "social practice" (p. 1) within the philosophical and political context that figures in Althusser's project and in "the vicissitudes of an intellectual tradition arising from it" (p. 1). That context, specifically Humanist Marxism and Stalinist Marxism, poses the "decisive question" (p. 20) for socialists today: How to link "Marxist science" (p. 20) and human liberation? How, in other words, can socialists realize Marx's Eleventh Thesis on Feuerbach: "The philosophers have only *interpreted* the world in various ways; the point is to *change* it." Benton's formulation of the decisive question reveals why realizing this link—the source of Marxism's *elan vital*—may be its fatal flaw. He writes, "Fail to resolve [the link] at all and theory has no relevance to socialist practice ('theoreticism,' Althusser calls this). Resolve it through imposition via a coercive party or state apparatus and you have Stalinism" (p. 20).

In Part 1, Benton incisively examines Althusser's "third way" (p. 19), the "project of rigorous problematisation and reconstruction of some of the most basic concepts and propositions of historical materialism" (p. 111). Benton's assessment strikes a cogent balance, identifying problems with and achievements of Althusser's early work (chaps. 2-4) and Althusser's subsequent self-criticism and revision (chap. 5). In Part 2, Benton trenchantly scrutinizes the work of several people influenced by Althusser. Focusing upon "a small number of selected fields . . . of importance both for the viability of historical materialism in social science, and for social theory as such" (p. 112), Benton offers discerning treatments of debates among Claude Meillasoux, Emmanuel Terray, P. P. Rey, and others concerning pre-capitalist societies; the work of Barry Hindess and Paul

Hirst on transition and articulation of modes of production; and of Perry Anderson, Charles Bettelheim, and Juliet Mitchell (chap. 6). In chapter 7, Benton discusses Nicos Poulantzas' work "both in the field of state-theory and in the class analysis of contemporary capitalist formations" (p. 143).

Chapters 8 and 9 contain Benton's reexamination of Althusserian Marxism in light of *Les Nouveaux Philosophes*—"The Rebellion of Subjectivity" as he puts it—and criticisms leveled at Althusser's project by E. P. Thompson, Anthony Giddens, and others ("Crusaders and Sociologists").

Benton's contextual, incisive inquiries are thoroughly animated by his engagement with the issues raised by Althusserian Marxism and responses to it. Althusserian Marxism and its ramifications are among the most important theoretical undertakings of the late twentieth century. Even readers who will not be drawn to accept Benton's conclusions will be drawn into his "settling of accounts" (pp. vi and 227) with these significant social phenomena. Readers working within a Marxian framework will find Benton's book a searching intervention into perennial socialist concerns via his discriminating engagement with Althusser and Althusser's influence. Non-Marxist social scientists will find Benton's book a helpful means (the book provides valuable notes, references, and indexes, including an index of Structural Marxist terms) of becoming conversant with a school of thought, a set of issues, and an ongoing debate about which no student of contemporary capitalist society can afford to be uninformed.

JAMES C. FOSTER

*Emory & Henry College*

**Liberalism Proper and Proper Liberalism.** By Gottfried Dietze. (Baltimore: Johns Hopkins University Press, 1985. Pp. vii + 282. $27.50.)

Dietze moves from the uncontroversial position that individual liberty is the essential emphasis of liberalism ("liberalism proper") to an exposition of "proper liberalism" as understood by these classical eighteenth-century liberals: Montesquieu, Adam Smith, Kant, and Jefferson. Utilizing his enormous knowledge of their writings and the commentaries on them, Dietze follows a similar format for each thinker. He shows that although liberty was a predominant value to each, they all thought the rule of law, a "moral minimum," must first constrain the liberty of individuals and then prevent the arbitrary and excessive authority of government. Each writer was thus a "man of measure" committed to a middle ground between anarchy and despotism.

The basic shortcoming of this book is that Dietze does not successfully show why the thought of these four writers constitutes "proper" liberalism. He is struck by their basic agreement in spite of their different circumstances. They wrote during "a period of moralism," and although they represented no "liberal establishment," their writings were "more mature" than earlier liberals (p. 50). Yet, among eighteenth-century writers, Bentham was arguably much more influential intellectually than Jefferson. A discussion of utilitarianism would raise important issues about liberty but would also weaken the agreement that Dietze finds among the writers selected. Most importantly, why should "proper" liberalism be found by examining only eighteenth-century writers? It is not clear why they are more authoritative than John Stuart Mill or T. H. Green or John Dewey. Moreover, although there have been important recent attempts to specify and to defend liberal principles, Dietze does not refer to such thinkers as Rawls, Nozick, and Dworkin.

At the end of the book we gain insight into Dietze's choice of the four writers. They had great respect for property rights and for the restraints on government necessary to protect these rights. Such rights and restraints have, Dietze believes, been weakened as contemporary Western societies move toward both anarchy and despotism. The despotic tendency is toward "a government that considers the unequal distribution of property an anomaly and will regulate economic rights through social legislation"; the anarchist tendency is toward "license and problems of law and order" (p. 266). That we are already far along the path to anarchy is clear in the Warren Court's "expansion of civil rights in an expanding democracy," which led to "people's assuming they could take the law into their own hands and turn against law and order" (p. 265). The trend toward despotism is established by Hayek, who "shows how liberal democracy became replaced by social democracy and how under the banner of democracy proper liberalism was discarded and the road to serfdom taken" (p. 260). Dietze offers little argument, however, in defense of his perspective on political events.

Dietze often does not examine problems concerning the interpretation of the four writers. For example, he states that Kant thought "freedom grows with autolimitation made in the awareness of duty" (p. 137), but he offers little explanation or defense of Kant's position. Nor does he respond to the argument that Kant's ethical theory is too formalistic to offer specific guidance for behavior. Dietze also does not explore the dispute as to whether Jefferson was caught within the individualistic "Lockean consensus" that Hartz thinks is dominant in American political thought or rather was committed to the ideal of virtue of classical republicanism, as Pocock argues. In conclusion, this largely untheoretical, highly expository book is most helpful as a source for statements on liberty and the rule of law by undoubtedly important eighteenth-century thinkers.

ROBERT B. THIGPEN

*University of New Orleans*

**The Politics of Socialism: An Essay in Political Theory.** By John Dunn. (New York: Cambridge University Press, 1984. Pp. xviii + 107. $29.95, cloth; $8.95, paper.)

This is a reader's joy: a brief but powerful book on an extensive and important topic whose scope is a reconsideration of the problem of socialist politics in developed nations, concentrating on why socialist politics remain attractive in such societies and whether in the future they can "still constitute a rational and civilising form of political enterprise" (p. xvi). Dunn's account is divided into three main sections that analyze what a successful socialist political theory would look like, how democratic socialism has in fact worked in practice, and whether a socialist politics is actually possible given the widespread opinion that practice has failed to fall even remotely short of the demands of theory. Dunn argues that, as a variety of political theory, socialism ought to explain reality satisfactorily, show us what forms of society are desirable, and give us some idea of how to realize in practice what we desire (while maintaining the better aspects of the societies we now inhabit, a salutary emphasis to which Dunn returns). His chief example in accounting for socialist success or failure is the 1983 defeat of the British Labour party, although his discussion of the debate accompanying this in Britain throws light upon many aspects of modern European politics.

Much of Dunn's first section is taken up with a discussion of socialist inabilities to speak clearly about political organization and about the assumptions and weaknesses of the Leninist model of the party, whose peacetime concentration of power and rigid conception of human nature and class representation are also attacked. Socialist politics nonetheless remain no less tentative and ambiguous than other forms of politics, and Dunn seems to imply that a liberal conception of party, with greater thrust placed in the participants and in the process of political education, could readily be assimilated to socialist goals.

In Part 2, Dunn distinguishes between three forms of modern social democracy: the first assumes socialism to develop automatically from capitalism, a second virtually disregards the need for any fundamental economic change (e.g., the modern German SPD), and a third places greater emphasis upon political democracy as a means of asserting the superiority of socialism over capitalism. Although the broadest popular appeal has often lain with the third type, it is the question of socialist economic organization and reform that has most bedeviled past socialist political strategies and that continues to present the most pressing problems. Yet it is in the realm of politics that electorates are persuaded that alternatives are available and that reform and restructuring are not utopian myths, and here it is political and rhetorical skills, rather than economic programs, that will provide the basis for any socialist future. Here the question is not whether more efficient forms of socialist economic organization might exist where "culture rules economics" (p. 69), but the fact that this is still widely regarded as implausible.

To persuade most that it is not, a goal so necessary to the securing of the trust of the working classes which Dunn sees as the main goal of socialist politics, it is necessary for socialists to reconsider how society might actually be improved, a far more difficult task than merely criticizing its present inadequacies. The complete socialist conception of the good, the utopian ideal that Dunn calls "true socialism," has essentially been lost; indeed, it is unclear that any institution might adequately realize it. This is not to insist that socialism must inevitably be tyrannical if its goals are even to be attempted properly, but it is massively to discredit the notion of socialism in which intellectuals impose their views upon society *carte blanche,* as Pol Pot most recently attempted. Correspondingly, too, it is to recommend socialist attention to nineteenth-century liberal emphases upon constitutional balances, the rule of law, political accountability, and the like. Without reverting to such mechanisms, and providing a far more compelling description of the advantages of economic planning, socialism does not have a future to which many will want voluntarily to subscribe. These, as Dunn puts it, "are not inspiring conclusions" (p. 93), but they demand our attention insofar as socialism is not likely to cease being seen as an alternative to capitalism for some time to come.

This is a very wise and judiciously reflective book on a topic of momentous importance. It repays reading and rereading, not only by students of socialist politics, but also by all observers of twentieth-century political development. In a large and too often dogmatic or pedantic literature, no other account as soundly reasoned and penetratingly observant is available.

GREGORY CLAEYS

*University of Hanover (West Germany)*

**Hegel, Heidegger, and the Ground of History.** By Michael Allen Gillespie. (Chicago: University of Chicago Press, 1984. Pp. xv + 217. $22.00.)

This thoughtful and stimulating work boldly takes on the task of assessing the thought of both Hegel and Heidegger. Gillespie seeks to explain how these two philosophers have tried to understand what history means when taken as a whole, and what significance history has for illuminating our essential characteristics, goals, and limits. Is history coherent, possessing some intelligible design, order, or goal? Or is it, as much human experience seems to attest, a story significant in parts but overall riddled with folly, slaughter, and caprice? If the former, then maybe history can be the source of answers to our most fundamental questions about ourselves and about society. If the latter, then we must seek an understanding of the fundamental questions by looking more to our ideas of nature, religion, or science rather than to the ups and downs of political events. Much recent thought holds that history is indeed more important as a guide for thought than nature, religion, or science. Gillespie wants to examine the deepest attempts to elucidate this idea of the importance of history, and so he rightly turns to those philosophers who explicitly face it.

Gillespie's book provides both a comprehensive overview of the political and philosophic orientation of Hegel and Heidegger and then also a more specific treatment of their attempt to fathom whether there is a "ground of history," whether it is based in something intelligible and coherent. Gillespie's account of the general outlines of the thought of Hegel and Heidegger is a marvel of clarity. The comments on Hegel (chaps. 2-4) seem particularly penetrating. Noteworthy is the vivid explanation of Hegel's political views (pp. 33-47, 85-103) and of Hegel's response to the problems suggested by Kant's work, and also Gillespie's rather detailed commentary on the "Introduction" to the *Phenomenology* (pp. 63-84). Almost equally acute and illuminating is the single lengthy chapter devoted to Heidegger, which contains a lucid account of Heidegger's reading of the history of thought. Gillespie's method throughout is to explain Hegel and Heidegger from within their own set of terms and questions. The book proceeds with no concessions at all to Anglo-American positivism; it does not acknowledge the distance that seems to prevail between the issues of concern to Hegel and Heidegger and the ones frequently uppermost in contemporary American philosophy or political science, and it does not attempt to bridge this gap.

Two observations might be made about the general story told in this book. First, it seems that we have today a better grasp of the origins and import of Hegel's work than of Heidegger's, and that scholarship handles his work more securely (which does not necessarily mean a complete understanding is readily available). Second, Heidegger's ideas seem somehow closer to our experience today than those of Hegel, even if strangely remote from politics; Heidegger emerges from Gillespie's comparison as the more compelling figure, although by no means wholly attractive.

Gillespie's special theme is the issue of whether there is an intelligible ground of history. "Ground" seems to mean a fundamental unity (pp. 55, 95); something is grounded when it is unified or whole in an intelligible way. Heidegger argues that it is Plato who first pursued the issue of a ground and made it "the central question of metaphysics and the West" (p. 142). In Gillespie's view, Hegel thought there was a "ground to history," and that history must be understood to be rationally ordered. After presenting the arguments very thoroughly, Gillespie suggests that Hegel's view is questionable (pp. xiv, 113-115), either because it is naively optimistic in its rationalism or, under the surface, darkly fatalistic. His remarks suggest that Heidegger is closer to stating in philosophically persuasive form what is found in the modern experience, when optimism, historicist progressivism, and rationalism seem all in decay. But from Heidegger we learn that there is no "ontological ground" for history or for anything else, indeed that the whole issue is an error, because the search for an absolute ground obscures an ostensibly more primordial, fertile insight into Being. This recognition, according to Heidegger, also helps to free us from the tyranny of technological ways of thinking, among which he includes the opinion that historical studies are somehow a key to interpreting what we are. Gillespie notes the strengths of Heidegger's view, while rightly calling attention to the almost inhuman distance from the "everyday realm of inauthentic life" that Heidegger establishes and that has much to do with his "woefully weak grounds" for teaching us how to deal with "the capricious and often demagogic claims of false prophets" (pp. 170-171). Gillespie also rightly ends this study by noting that the great questions are illuminated but not solved by Hegel and Heidegger and remain for us to address.

DONALD J. MALETZ

*The University of Oklahoma*

**Hayek on Liberty.** By John Gray. (New York: Basil Blackwell, 1984. Pp. x + 230. $29.95.)

This book offers a thorough explication of one of this century's most prominent classical liberals. Its basic theme is that Hayek's various specialized contributions to legal, social, and economic theory have emerged from a coherent system of thought, a specific philosophical outlook.

What is the system of ideas that characterizes Hayek's thought? Gray at first labels it a "sceptical Kantianism" that explores the limits of reason through immanent criticism. However, most of the discussion focuses on other influences on Hayek's views. Prominent among these influences are Popper, Wittgenstein, J. S. Mill, and especially, Hume. Indeed, by the time we reach the end of the book references to Kant are all but forgotten, and Hayek's contribution is to have returned "thought about man and society to the great tradition of the Scottish Enlightenment. . ." (p. 140).

Several concepts are found to be central to Hayek's system of ideas. One of Hayek's basic beliefs is that social order is spontaneous. It is not the product of conscious control or rational design; it is instead a fortuitous consequence of human action. Allied with the concept of spontaneous order is an epistemology that views most of knowledge as tacit or practical in character, and some of it as inarticulable. Attempts at social planning (based as they are on abstract, theoretical knowledge) will thus be limited in success, if not entirely self-defeating, for they run counter to the wisdom embodied in our practices.

For Hayek, then, social institutions and rules of conduct are the vehicles and repositories of human knowledge. Complexes of practices and rules (i.e., traditions) are in a competition that filters out error and brings us closer to an approximation of truth. Hayek ultimately evaluates social orders by means of an indirect utilitarianism that applies the principle of utility "not to specific practical questions, but to whole systems of rules or codes of conduct" (p. 96). Such applications of utility have led Hayek to seek the economic and juridical frameworks necessary for ensuring the growth (and to some extent, the prevention) of decentralized, practical knowledge.

Gray's book offers us a valuable comprehensive look at the philosophical basis of Hayek's social thought. It offers relatively detailed illustrations of how Hayek's views on law, morality, and economic theory are related to a well-grounded philosophical system. Gray's concluding chapters also provide interesting comparisons between Hayek and other social thinkers, as well as an extended discussion of some common criticisms of Hayek's positions. And, not least among the book's accomplishments, Gray gives us a useful, extensive bibliography of works by and about Hayek.

However, there are important defects in what is otherwise an excellent synoptic glance at Hayek's thought. First, Gray shows little awareness that theorists other than classical liberals have also held organic and/or evolutionary views. Diverse thinkers as Comte, Hegel, Marx, and Toulmin have all had such perspectives but have derived substantially different conclusions regarding preferred social orders or the growth of knowledge. As it is, Gray explores only those theoretical heavyweights who support the classical liberal perspective, which results in a sanguine appraisal of Hayek's political and economic recommendations.

The book also suffers from apparent incompleteness. For example, political theorists seeking Hayek's views on the preferred political system will find only a handful of pages devoted to one of Hayek's chief recommendations for guaranteeing liberty, namely, bicameralism. But more importantly, Gray provides little sense of what is to be gained from viewing Hayek's thought as a "synthesis of Humean indirect utilitarian argumentation with a Kantian test of universalizability" (p. 96). Indeed, the only payoff discussed is a paradigm shift that abandons the dead ends of conceptual analysis and social choice theory in favor of Hayek's perspective on spontaneous social order and evolutionary epistemology. In that case, however, other currents of contemporary thought have crossed the finish line ahead of Gray's interpretation of Hayek.

LEONARD WILLIAMS

*Manchester College*

**Prophets of the Left: American Socialist Thought in the Twentieth Century.** By Robert Hyfler. (Westport, Conn.: Greenwood Press, 1984. Pp. x + 187. $29.95.)

Robert Hyfler has written a tightly argued, at times eloquent, study of American socialist theorists. His book is not intended as a comprehensive history of twentieth-century American socialism or even of all significant socialist thinkers in this century. He somewhat arbitrarily selects a small group of thinkers who espoused different versions of a socialist vision while working for realizable goals. Hyfler pays some attention to the biographical details of his "prophets'" lives and political activities. His main interests, however, are their theoretical contributions and the contribution of their theories to the failure of American socialism.

Hyfler does not, of course, blame that failure entirely on theoretical shortcomings. "Wilsonian repression" (p. 90) helped destroy the promising leadership of Eugene Debs. When antiradical forces again grew strong after World War II, they greatly weakened the organized Left and cut off potential new socialists from their theoretical heritage. Thus, even the student Left of the 1960s could not readily build on or react to the theories of earlier American leftists.

Hyfler's most interesting argument is a complex analysis of the links between labor activism and socialist theories. He contends that the typical view of Samuel Gompers as the conservative leader of America's nonsocialist labor unions misses an important element of his role. Gompers urged workers to seek economic gains through their unions rather than political gains through a socialist party. However, Gompers's opposition to a politically oriented labor movement often reflected his belief that government served the interests of capitalists more than it reflected antisocialism per se. Hyfler further contends that Gompers and other "working-class accommodationists" emphasized "the self-liberation of the working class" (p. 59), even while they believed that workers were not strong enough to control the state.

Hyfler argues that those socialists who shared Gompers's belief in conservative tactics held a sharply different theoretical perspective. For example, Morris Hillquit, a founder of the United Hebrew Trades and leader of the Socialist party, favored tactics as moderate as those of Gompers. Yet the elitist and statist nature of his socialism distanced his theories from Gompers's beliefs. Hillquit believed that socialist leaders must dominate the working class, which could not be expected to liberate itself. Moreover, he believed that socialists could peacefully influence or capture the state and use it in the interest of workers.

Other socialists had theoretical beliefs that partially paralleled Gompers's, but they did not support his conservative tactics. Debs did not reject political activism but shared Gompers's suspicions concerning the state in a capitalist society. He also believed that the working class had a great potential for self-liberation. Yet Debs thought this potential should be used to develop socialist class consciousness. He believed that Gompers's American Federation of Labor was impeding the formation of class consciousness because it was organized along narrow craft lines. Debs favored industrial unions that would unite workers organizationally and help them see their common interests.

Hyfler gives briefer treatment to more recent developments in socialist thought. He does note the schisms that emerged on the Left after the initially broad approval of the Bolshevik Revolution gave way to disillusion on the part of many socialists. Hyfler also devotes a short chapter to Norman Thomas that minimizes his theoretical significance. He concludes by questioning whether Michael Harrington's democratic socialism is sufficiently socialist. This last argument is not entirely convincing, but Hyfler's generally perceptive analysis of his prophets deserves attention.

SANFORD N. GREENBERG

*Austin, Texas*

**The Islamic Conception of Justice.** By Majid Khadduri. (Baltimore: Johns Hopkins University Press, 1984. Pp. xvi + 256. $25.00.)

This latest of many works by Khadduri is an attempt to "inquire into the nature and scope of Divine Justice and to reconstruct from the diverse writings of Muslim men of learning the Islamic theory of justice in all its aspects" (p. 11). In so doing, he has provided us with a study of great breadth as well as depth; not only is justice a central concern of Islamic thought, but it also has been inextricably linked with other persisting issues. It would thus be difficult to identify many major Islamic thinkers—particularly from the medieval period—that have escaped Khadduri's attention.

One unique facet of this study is the author's classification of the aspects of justice dealt with by Islamic thinkers. He begins with "political justice," which relates to the question of legitimate succession that split the Islamic world into sects and also to the doctrines of free will and of the right to accuse proclaimed Muslims of being unbelievers, both of which were rejected by supporters of impious Umayyad rulers.

Under the rubric of "theological justice," Khadduri proceeds to the more elaborate later debate between the Mu'tazilites and their opponents over free will and determinism and over reason and revelation, as well as to the positions of various sects and of Sufi writers on these issues. Then he focuses on "philosophical justice," defined as the effort to understand "justice as presented in the works of Greek philosophers and to make it intelligible to believers" (p. 78)—particularly as demonstrated by four great thinkers, al-Kindi, al-Farabi, Ibn Sina, and Ibn Rushd. Another chapter deals with a rich body of literature on "ethical justice," involving standards of conduct that go beyond the minimal requirements of the law and that are characterized by an effort "to harmonize Islamic with foreign notions and values" (p. 106). Under the heading "legal justice," the underlying principles of both substantive and procedural justice are identified, while a chapter on "justice among nations" is devoted to the medieval theory of an Islamic world that is in a state of war with the rest of the world and particularly to an equation of the concept of *"jihad"* with the Western tradition of "just war." A final category is "social justice," which is said only sometimes to include distributive justice and the essence of which is identified with promotion of the "public interest" by improving social conditions and consequently the health of the state—an idea whose fullest development is credited to Ibn Khaldun.

Most of the book deals with the medieval period. Although Khadduri emphasizes the continuity of basic questions, he somewhat incongruously relegates treatment of the modern era to the last chapter. Stress is put on conflict between Revivalist and Modernist schools of thought and on the alternation of "complementary" trends, with "so-called Islamic upsurges" not necessarily opposing innovation but providing "a breathing space for society to assimilate" change (p. 229).

Only at the risk of being considered picky can one single out even minor substantive flaws in this admirable work. As cases in point, Khadduri's attribution of the difficulty of "adjustment to secularism" to Islam's being a young religion (p. 196) is hardly satisfactory, and "separation of church and state" (p. 202) is an imprecise description of what Ataturk carried out in Turkey. Also, Khadduri contradicts himself with regard to whether Khumayni rises above "denominational differences" (pp. 226-227). Both the high quality of this volume and the absence of any other comparable broad studies ensure its long-lasting value to students of the Islamic world and to those in many disciplines who are concerned with conceptions of justice.

GLENN E. PERRY

*Indiana State University*

**The Radical Politics of Thomas Jefferson: A Revisionist View.** By Richard K. Matthews. (Lawrence: University of Kansas Press, 1984. Pp. ix + 171. $22.50.)

Because Jefferson expressed his thoughts over a very lengthy course of time and in a body of writing that is as unsystematic as it is voluminous, there is both license and evidence to read back into the expression of his ideas any tradition that one might want to. Most of the time, Jefferson is depicted as one kind of a Lockean liberal or another. Richard K. Matthews wishes to revise this estimate by showing how Jefferson's thinking is indicative of an altogether different tradition. What results from his effort is a decidedly "New Left" Jefferson. Others have argued that Jefferson is a far more radical political thinker than the conventional literature on him, or the dominant culture's image of him, acknowledges. But no one, to my knowledge, has searched out this alternative Jefferson in order to state his own quarrel with liberalism with the care and forcefulness that Matthews shows in this well-written book.

To wrest Jefferson from Locke, Matthews reads the former with the assistance of Gary Wills and J. G. A. Pocock. He also makes some very interesting uses of Hannah Arendt's views on Jefferson's ward republic and of Leo Marx's "middle landscape" concept from Marx's classic study of technology and the pastoral idea in America. However of all the guides to whom Matthews acknowledges indebtedness, the most influential is C. B. MacPherson.

Carefully examining familiar bits and pieces of Jefferson's writing from the diverse angles of perspective that he borrows from those cited previously, Matthews uncovers what he regards as the central elements of Jefferson's political thought: civic-humanism, communal anarchism, and radical democracy. Civic-humanism is based upon what is shown to be Jefferson's republican critique of market society relations and his preference for that "middle landscape" in which one could be antimarket without being antimodern. Communal anarchy takes form from the anthropological lessons Jefferson learned from first-hand study of his own Indian neighbors. These lessons regarding man's naturally social constitution, combined with Jefferson's unflaggingly optimistic appreciation of the unique import of New World conditions, convinced him of the possibility of living out from under the shadow of the leviathan without reverting to primitivism. Finally, accepting Jefferson's insistence that the earth really does belong to the living, Matthews demonstrates certain affinities between Jefferson's thinking and more recent concepts and expressions of participatory democracy and permanent revolution.

The argument regarding each of these chief elements in Jefferson's political thinking remains close to the text while deftly drawing on and melding the insights of others. Each of these arguments is closely articulated with the other two. Along the way, Matthews makes some very nice distinctions between Jefferson's thinking on the one hand and that of Rousseau, Paine, Madison, and Hamilton on the other. Madison is shown to be closer to Hamilton than Jefferson, and Jefferson, in turn, to the left of Paine.

It will take a far longer book to convince some that Jefferson belongs to and shaped that tradition of politics from which the New Left springs. However, after reading this book, no one will dispute the fact that Matthews demonstrates the value of Jefferson for those who might wish to fashion an American alternative to Lockean liberalism.

DARRYL BASKIN

*Elmira College*

**Fortune Is a Woman: Gender and Politics in the Thought of Niccolò Machiavelli.** By Hanna Fenichel Pitkin. (Berkeley: University of California Press, 1984. Pp. ix + 345. $24.95.)

In this multifaceted book Hanna Pitkin offers us a fundamentally new reading of Machiavelli. Through primarily a close and often brilliant exegesis of Machiavelli's writings (including his plays, poetry, and correspondence), she offers us an account focused on the gendered nature of Machiavelli's central images and on the problems that these images create in his treatment of the issue of human autonomy.

To complement her outstanding skills in exegesis, Pitkin brings two other methods to bear on her interpretation: (1) a cautious application to Machiavelli's work and times of lessons to be learned from selected psychoanalytic literatures; and (2) an examination of the relevant research concerning sexual relations, marriage, and childrearing in Renaissance Florence. In addition, she closes the book with a powerful, extended meditation on the present relevance of some of Machiavelli's insights, a meditation on what she sees as the tasks and themes of contemporary political theory.

In the Introduction, Pitkin identifies the problem of autonomy as Machiavelli's "central preoccupation" (p. 7). It is also her own main preoccupation, as becomes very clear in her concluding "Meditations on Machiavelli." What Machiavelli "at his best," as she puts it, is able to teach us about politics is the need for an autonomy "that acknowledges our necessary interdependencies" (p. 324), an autonomy that involves the development of our "capacity for mutuality within difference" (p. 301). However, for Machiavelli autonomy is all too often "intertwined" with manhood—and hereby hangs the tale. If autonomy is synonymous with manhood, that which threatens autonomy comes to be identified as female. But the female cannot be expunged from the human condition, nor can it be expunged from the individual male psyche. To define autonomy against the female thus becomes self-defeating. The outcome of what Pitkin calls the "misogynistic" definition of autonomy (p. 236) is the creation of strong father images and military discipline—that is to say the destruction of the community of autonomous and mutual citizens advocated by Machiavelli "at his best."

Machiavelli's conflicting images of manhood include the "Fox," the "Founder," and the "Citizen" (all traced in Part 2). It is the interplay of the last two that provides Pitkin's focal point. Founder images are misogynist, whereas Citizen images could be interpreted without gender bias. The Founder, or restorer, is an "armed prophet." Generally a motherless foundling, he brings about manly *virtù* in the citizens by his ruthless but necessary cruelty, which alone can defeat the female forces of corruption within and without. But in so doing he "destroys his sons" (as Brutus literally did), reducing them to a passive obedience that is the opposite of autonomy. By contrast, the Citizen image of manhood implies "peer mutuality" and "fraternal rather than paternal and filial" relations (p. 90). But it seems impossible that the citizens of Republican *virtù* that Machiavelli calls for could emerge from the actions of the Founder.

The all-powerful female forces that must be resisted are examined in Part 3: women as sexual corrupters of men, sowers of discord, malicious shrews—the female personification of Ambition and Avarice as well as of Fortune herself. Pitkin goes on (Part 4) to point out that such images were pervasive at the time. Evidence from historians suggests that for Florentine women life was becoming increasingly confined to the home, and psychoanalytic theories suggest that increasingly intense mother-child relations probably resulted, leading to heightened male ambivalence towards women. Returning to Machiavelli's texts, Pitkin then shows us the operation of this ambivalence in Machiavelli's accounts of the "birth" and "rebirth" of Rome and Florence—accounts of *men* giving birth to cities, accounts in which autonomy is conceived as *male* autonomy, "won from and sustained against female power" (p. 230).

Pitkin's final "meditation" (Part 5) attempts to rescue Machiavelli from himself: There is a vision of universal autonomy and mutuality in the Citizen image, even though Machiavelli never manages to sustain it. It is upon this image that Pitkin claims we must build. Machiavelli originates a meditation on the relations of human autonomy to action, to membership in political communities, and to principles of judgement. In Pitkin's reading, Machiavelli becomes an anticipator of Kant (and, it seems to me, more generally of the liberal tradition), identifying autonomy with choosing one's own principles of action (p. 315) and advocating the recognition and resolution of conflict within mutually agreed limits.

There is plenty to disagree with in this book, particularly in Pitkin's own vision of politics in the final "meditation." Equality, for example, is not, I think, adequately addressed within the concepts of membership and mutuality. Machiavelli scholars will doubtless find fault with Pitkin's interpretation of various passages. But this is a book of such power and breadth that it will not be diminished by such criticisms. It is also a book that stands as an important indicator of the profound impact that feminism has now had on the writing of political theory. It demonstates that by using new lenses developed by feminist scholars, by focusing on themes that would once have seemed irrelevant or subordinate to more explicitly "political" matters, we can in fact profoundly and fruitfully modify our vision of the political theory of a major figure in the Western tradition.

SONIA KRUKS

*New School for Social Research*

**Locke's Education for Liberty.** By Nathan Tarcov. (Chicago: University of Chicago Press, 1984. Pp. viii + 272. $22.00.)

"Politics," John Locke wrote in 1703, "contains two parts very different the one from the other, the one containing the original of societies and the rise and extent of political power, the other, the art of governing men in society." Without acknowledging authorship, Locke went on to recommend his *Two Treatises of Government* as one of several books on the first part of politics. As to the part "which concerns the art of government," he said, "that I think is best to be learned by experience and history. . ." (*The Educational Writings of John Locke,* ed. James Axtell, Cambridge University Press, 1968, p. 400).

Nathan Tarcov's argument is that Locke offers more guidance on "the art of governing men in society" than the bare admonition to look to experience and history. According to Tarcov, Locke's educational writings, especially *Some Thoughts Concerning Education,* supplement the *Two Treatises* by supplying insight into Locke's views on this subject. "Although one must be very careful in this regard," Tarcov says with characteristic caution, "one can conclude that Locke's account of the art of governing children aids in the task of understanding what he might have written on the second part of politics" (p. 7).

Before he turns to the *Thoughts Concerning Education,* though, Tarcov analyzes at length (65 pages) and in detail (407 notes) Locke's defense of the natural freedom of mankind against Filmer's patriarchal arguments. Indeed, Tarcov directs his scrutiny not only to Locke's *First Treatise* and Filmer's writings, but also to other writers in whom Filmer detected the notion that men are born free from all subjection—Bellarmine, Calvin, Buchanan, Barclay, and Hobbes among them—in order to clarify the meaning of the doctrine of natural freedom. This circuitous route leads to the conclusion that Locke sharply distinguishes between the proper roles of civil government and the family. As Tarcov puts it, "Education, which Locke tells us in the *Essay* and the *Thoughts* is more responsible for making men what they are and even exercises more influence over their conduct than do civil laws, he entrusts exclusively to the family, the authority that lacks the power of life and death. One is tempted to say that that is the fundamental separation of powers" (p. 72). The task of government, then, is to preserve life, liberty, and property, while the family acts as "the agency of Locke's education for liberty" (p. 76).

In what is this education for liberty to consist? To answer this question, Tarcov devotes the remainder of his book to a close reading of the *Thoughts.* There he discovers a Locke who proposes to use the innate desire to be free to instill in the gentry the virtues of self-denial, civility, and liberality. Locke is a moralist, it seems, one who provides both a "complex account of how to build concern for the life, liberty, and property of others on a solid basis in human nature" and a "subtle account of the nature of the human attachment to liberty and how to nurture and direct it" (p. 183). He is, moreover, a moralist whose morals may well be pertinent today. "His emphasis on the harsh virtues of self-denial, courage, hardiness, and industry may offend our easy-going self-gratification," Tarcov writes in his conclusion, "but these virtues may still be necessary to the individual liberty and comfort that we join with him in valuing" (p. 210).

There is much to admire in *Locke's Education for Liberty.* Tarcov's command of the relevant literature is impressive, his reading of Locke is both sensitive and sensible, and his views are expressed clearly throughout the book. My only complaint is that Tarcov attends so assiduously to Locke's texts that the reader loses sight of the fact that Locke lived and wrote, for the most part, in seventeenth-century England and Holland. One can turn to James Axtell's fine edition of the *Thoughts Concerning Education,* however, to gain an appreciation of the context. For an appreciation of the moral and political implications of Locke's writings on education and the family, and of what they reveal about his views on "the art of governing men in society," we can ask for no better guide than Nathan Tarcov.

RICHARD DAGGER

*Arizona State University*

**The Political Thought of Martin Luther.** By W. D. J. Cargill Thompson. Edited by Philip Broadhead. (Totowa, N.J.: Barnes & Noble Books, 1984. Pp. xv + 187. $26.50.)

This book, an edited version of an almost complete manuscript found among the author's papers after his death, is the work of a first-rate scholar. Cargill Thompson was a distinguished ecclesiastical historian, and his extensive knowledge of the reformation's theological and political controversies allowed him to interpret Luther's always polemical "tracts" in their proper context. More than that, he knew how and took the time to read Luther carefully, to follow his arguments, and take them seriously.

For Cargill Thompson, Luther's thought is worth understanding because he was the Christian extremist *par excellence.* Luther viewed himself as an Augustinian, radically distinguishing between spiritual and temporal, faith and reason, Church and politics, voluntary and coercive, word and sword, inward peace and external peace, and so forth. The idea of the two realms of human existence or "the two Kingdoms, with all of its ramifications, is the key to Luther's mature social and political thought" (p. 42). Luther's articulation of this idea was considerably more clear, complex, and concrete than that of Augustine. For Luther, this fuller articulation was necessary to oppose the medieval, Thomistic propensities to synthesize and order hierarchically, to show why true Christianity stands radically in opposition to the comprehensive rationalism of Aristotelian ethics and metaphysics.

Luther always claimed to have deduced his political thought from his theology. This constancy gave his diverse political opinions a certain inner consistency, but this consistency is perhaps only clear from Luther's own perspective. His political opinions did change markedly over time.

Luther's early thought is based on a simple and powerful faith in the word as *the* effective means for the reformation of human affairs. Political compulsion is useless. It only produces the hypocrisy of outward conformity without genuine reformation or change of heart. Consequently, the early Luther was a partisan of complete religious toleration.

He held that the political realm was charged with only the production of external peace. It has no concern with the soul. As a result, Luther taught that "secular princes *qua* princes had no authority in religious matters and ought not to impose faith on their subjects or to legislate in religious matters" (p. 56). But, following a powerful medieval opinion, Luther also believed that the Ten Commandments were all in accord with reason or natural law. It is clear they include a prohibition against blasphemy, the public expression of false belief. Political experience seems to have made it clear to Luther, in spite of his theoretical extremism, that the word's effectiveness might be indebted to political power. He gradually expanded his definition of blasphemy. Eventually, "the dividing line between public blasphemy and private error had become exceedingly narrow and Luther had, in effect, completely abandoned the remarkable 'liberalism' of his original position" (p. 162).

Students of political philosophy will wish that Cargill Thompson had said more about the contribution of Luther's thought to the development of modern liberalism. I find that what he says about Luther is extremely suggestive in this regard. I note, for example, that Locke agrees with Luther that reason or natural law is limited to understanding what is temporal or "external." Consequently, Locke teaches that political rulers have no competence in religious matters. He goes on to reject Luther's medieval prejudice in favor of the reasonableness of all the Biblical commandments. Locke, with Luther, concludes that Christian extremism properly understood is the doctrine most favorable to the understanding and achievement of true liberty.

As the founder "of a distinctly protestant political theory," Cargill Thompson says, "Luther's influence on man's attitudes toward the state" in some ways was "far greater than that of Machiavelli" (pp. 6-7). For this reason, both Luther's theory and Cargill Thompson's book deserve our serious attention.

PETER AUGUSTINE LAWLER

*Berry College*

**The Nursing Father: Moses as a Political Leader.** By Aaron Wildavsky. (University: University of Alabama Press, 1984. Pp. xi + 262. $25.00.)

The title of this remarkable book is taken from the eleventh chapter of the Book of Numbers where Moses complains that he must carry the Lord's people "as a nursing father beareth the sucking child." Wildavsky uses the image of

Moses as nursing father to present an analysis of political leadership based on the Torah. The book has two theses: "first, that understanding of the Mosaic Bible may be enhanced by treating it as a teaching on political leadership; second, that our understanding of leadership may be improved by considering it as an integral part of different political regimes" (p. 1).

An introductory chapter explains the book's plan and enables Wildavsky to position himself with regard to both the social science literature on leadership and contemporary biblical scholarship. The next five chapters pursue the theme of leadership as a function of regime. Wildavsky discerns four distinct regimes for the Israelites during Moses's lifetime: the regime of slavery under Pharoah, the regime of anarchy once they are out of Egypt, the regime of equity in response to the idolatrous worship of the Golden Calf, and the regime of hierarchy after Korah's rebellion. Each regime is presented as a model with its own salient characteristics. For example, in the regime of anarchy, the religious principle is "different gods for different purposes" and leadership is "meteoric"; in the regime of hierarchy, leadership is autocratic and religion monotheistic. Wildavsky shows how Moses leads in a different way in each of these regimes.

A concluding chapter critically reviews leadership literature and finds that "there is almost no discussion of types of political systems as productive of different kinds of leadership" (p. 190). Wildavsky deplores this neglect and argues that biblical studies will redeem leadership literature from its erring ways. The book closes with an appendix that speculates on how Judaism survives and finds the answer in a judicious blending of the characteristics of the regimes of equity and hierarchy.

As a study of leadership, *The Nursing Father* is a useful addition to the literature, but it falls far short of Wildavsky's ambitious goal to "supersede [leadership literature] with a perspective in which leadership becomes a part of regimes" (p. 26). As social science, this book will interest and inform the reader; but no more.

A quite different judgment is in order on *The Nursing Father* as an effort to enhance our understanding of the Pentatuch. Here the book will charm, delight, and challenge the reader. Wildavsky's warmth, wit and obvious affection for his topic attest to the vitality of the biblical tradition and its enduring capacity to address serious issues of any day, including our own. The book's dazzling success in offering a political interpretation of the Torah may cause the reader to forget whatever he might have learned about leadership. It is a small price to pay. The strength of this book lies in Wildavsky's profound political meditation on the biblical heritage. He does not teach the reader about the Bible, but how to learn from it. *The Nursing Father* is an eloquent bulwark against the tendency to collapse the rich relationship between religion and politics into trivial quibbles over church and state. Wildavsky's robust treatment of religion and politics provides hearty fare to replace the watery gruel of shallow legalism.

JOHN A. ROHR

*Virginia Polytechnic Institute and State University*

**Kant's Political Philosophy.** By Howard Williams. (New York: St. Martin's Press, 1983. Pp. xii + 292. $30.00.)

Howard Williams has two objectives in this book: "to provide an introduction to Kant's political philosophy," and to provide a "picture of Kant's political theory as a whole, [showing how Kant's] views on politics square with the major tenets of his critical philosophy" (p. vii). Williams accomplishes the first objective with great success, but his approach to the second goal seems less convincing, particularly in light of recent Kant scholarship.

Williams provides a good overview of the corpus of Kant's political writings. His discussions of Kant's views of property, women, and punishment are especially interesting. Williams is successful in demonstrating the link between Kant's ethics and his politics by way of his discussion of Kant's philosophy of history. Williams shows that the moral lesson in Kant's philosophical view of history is best understood when we look at man's relationship to history and nature as if this relationship were the ground for man's realization of the moral objectives of practical reason, the hope of a society where persons can be treated as ends and not solely as means. This relationship is characterized by man's rejection of nature's domination (or the domination of impulse) as he asserts his desire for self-discipline and for the deliberative qualities that permit him to create circumstances (republican government and the hope of an international order dedicated to peace) compatible with the goals of morality. "By seeing history as embodying a purpose the ethical person can enter the seemingly amoral quagmire of politics, and take advantage of its arbitrariness and unpredictability, to bring about a moral end" (p. 22).

But as Williams demonstrates, Kant paints a picture of citizens who are not motivated by the objectives of Kant's ethical philosophy, and yet Kant's politics is still committed to realizing these objectives. The question one could ask is how a politics that seeks ethical goals is possible in a setting where persons do not make ethical motives central to their lives?

Williams would reject as an answer to this question a view that makes Kant's philosophy of history historicist (p. 263). Williams understands Kantian citizens as holders of interests, and he shows how Kant's politics are formed in response to them. On this view, citizens, not history, shape politics. Williams's account centers on the nature of the interests that citizens hold. For Williams, Kantian citizens act and think as "egoistic commodity owners" (p. 194). Kant is said to be the proponent of *laissez-faire* liberalism (p. 194), where the state secures the right of a person to the "single-minded pursuit of his own self-interest" (p. 194).

Still, Williams's account is balanced, and he demonstrates that, for Kant, the right to pursue individual interests is sanctioned by the society and thereby this right always implies an obligation to certain social rules. Thus Williams correctly points out that Kant "realizes . . . that the individualist pursuit of liberty and equality has to be tempered by social restraints if it is not to lead to the destruction of civil society itself" (p. 194). The state requires that citizens respect certain constraints to guarantee equal liberty. At the same time, the state itself is organized to be a "balanced mechanism" that can insure citizen compliance with laws beneficial to the society as a whole because it (the state) is made subject to various limitations as well (p. 195). These limitations prevent the state from becoming a force that seeks to aggrandize power at the expense of citizen liberty.

But where citizens are motivated by non-moral concerns, would the state structure be sufficient to protect the liberty of all citizens? Patrick Riley (*Kant's Political Philosophy,* Rowan & Littlefield, 1983, pp. 9, 12, 14) argues that for Kant, as citizens act within the law of republican states even from motives of self-interest, they nonetheless manifest a commitment to support laws that have as their objective a concern for respecting the freedom of others. On Williams's view the basis for attaining the objectives of morality is the inherent optimism central to Kant's thought. "Finally, the success of Kant's whole political philosophy rests on the hope and moral presupposition that the human race will improve ethically" (p. 278). Still, Riley's view is more compelling because, unlike Williams, Riley does not make Kant's case for a politics that moves toward moral goals wholly dependent on Kant's feelings of optimism for the future. Instead, Riley demonstrates Kant's reasons for this optimism; namely, that in republican states persons act as moral beings even if for non-moral motives. Here a moral politics is thinkable even without the ethical advance of the human race.

STEVEN M. DELUE

*Miami University of Ohio*

# Empirical Theory and Methodology

**From Knowledge to Wisdom: A Revolution in the Aims and Methods of Science.** By Nicholas Maxwell. (New York: Basil Blackwell, 1984. Pp. vi + 298. $34.95.)

A work concerned with a revolution in the aims and methods of science requires an author who possesses a command of a number of disciplines. On the basis of this criterion, Nicholas Maxwell—a lecturer in the history and philosophy of science at University College, London, who also publishes in journals of physics and philosophy—is up to the task. I have presented his academic credentials because few political scientists are likely to be familiar with Maxwell's work and to point to a paradox in the final chapter, "The Revolution Is Under Way," of this call to action. This paradox is that although one might argue that Maxwell has been quite successful within the structures of both the university and academic philosophy, he is highly critical of the basic intellectual assumptions upon which opportunities and rewards are distributed in these structures. In his own words, "I am myself well aware of just how potently the philosophy of knowledge, as a result of being built into the institutional structure of academic inquiry, operates to censor out of existence work that fails to conform to its edicts. I have encountered this again and again in my own work—above all in my attempts, during the past ten to twenty years, to communicate and publish the proposals and arguments of this book!" (p. 277).

Maxwell's criticism is not that one methodological or theoretical paradigm (e.g., Marxism, behaviorism, quantitative analysis) has hegemony within the academy, but rather that the basis upon which each of these is built, the philosophy of knowledge, is irrational and must be replaced by a philosophy of wisdom. The basic difference between these philosophies is: "Whereas for the philosophy of knowledge the fundamental kind of rational learning is acquiring knowledge, for the philosophy of wisdom the fundamental kind of rational learning is learning how to live, learning how to see, to experience, to participate in and create what is of value in existence" (p. 66). The stress on objective knowledge rather than the pursuit of human values emerged in the sixteenth and seventeenth centuries with the rise of modern science and dominates scientific inquiry today.

The dominance of the search for knowledge rather than wisdom has a number of the implications for the social sciences. The term social *science* is a typical philosophy of knowledge misnomer. The proper term, from the philosophy of wisdom perspective, is social *inquiry.* The primary goal of political inquiry is to help us to articulate political problems and problems associated with government and to propose and criticize possible solutions, thus helping us gradually to realize how we can approach these problems in a more cooperatively rational manner. In a similar manner, the basic task of the study of international relations is to help humanity to articulate global problems and to propose and criticize possible solutions, so that gradually we may develop rational approaches to global and international relations. The primary problems are not the disciplinary based intellectual problems, for example, sociologists will be more concerned with social problems than with sociological problems. Finally, the philosophy of wisdom holds that social inquiry, with its concern with problems of living, is more intellectually fundamental than the natural sciences, which are concerned with secondary problems of knowledge.

Although Maxwell argues that the development of a rational inquiry based on the philosophy of wisdom is a necessary condition for the emergence of a saner, more just and humane world, he recognizes that it is not a sufficient condition. Interest and power groups, both internal and external to the academy, are likely to resist the change called for in the revolution from knowledge to wisdom. Maxwell's position here is that the ideology of the philosophy of knowledge is not likely to lead to a questioning of the aims, rationality, or morality of science, and that "a society in which there is a tradition of rational discussion of its problems has open to it all sorts of desirable possibilities—in particular the possibility of democratic, non-authoritarian, cooperative action—not open to a society in which there is no such tradition" (pp. 161-162). It is worth a try. However, Maxwell never does come to grips with the issue of the interests of the wealthy and powerful in determining the prevailing philosophy of our age. Marx's observations concerning the relationship between the ruling ideas and the ideas of the ruling class may be a good starting point for one interested in the politics of knowledge and wisdom.

This book may remind the reader of some of the concerns that attracted him or her to the world of ideas in general and to political science in particular. My one specific recommendation for potential readers is that they should have a familiarity with the work of Karl Popper and Thomas Kuhn before they read Maxwell.

MARK OROMANER

*Hudson County Community College*

**The Spiral of Silence: Public Opinion—Our Social Skin.** By Elisabeth Noelle-Neumann. (Chicago: University of Chicago Press, 1984. Pp. xi + 184. $20.00.)

Elisabeth Noelle-Neumann's *The Spiral of Silence* begins with a relatively simple task: how to explain an apparent anomaly in data on the voting intentions of the German electorate. It concludes as an elegant theory of public opinion. The book is an important contribution to the literature on voting behavior in particular and to public opinion research in general. It ought to be on the "must read" list of any social scientist interested in public opinion formation. Whether the spiral of silence constitutes a sufficient paradigm for public opinion research, as Noelle-Neumann suggests, will need to be determined by empirical applications.

While analyzing data for the German election of 1965, Noelle-Neumann, the director of the Public Opinion Research Center in Allensbach, Federal Republic of Germany, observed that voters began to believe that one party would win before their own vote intention changed. Why would voters who were intending to vote for one party come to believe that the other party was going to win the election?

Noelle-Neumann concluded that the question could be answered by reference to what she calls "the spiral of silence." In essence, a climate of opinion developed in Germany in which voters who supported the SDP felt increasingly uncomfortable about expressing public support for their party. As SDP supporters felt increasingly isolated, they expressed support for their party even less. And, ultimately, the threat of isolation and pressure to conform led voters to change their vote intention.

After a very few pages dealing with the electoral data that first piqued her curiosity—those pages are really more in the nature of an introduction—Noelle-Neumann turns to a much broader inquiry into the nature of public opinion. Her obvious intention in this enterprise is to provide a basis for the conclusion that the threat of isolation is the primary driving force in public opinion formation.

I felt most comfortable with the author's references to empirical data. The former involved questionnaire items that were replicated in a number of surveys as well as a quasi-experimental design called "the train test." The results of this test are of interest given a debate going on today in the U.S.: "After being threatened verbally, smokers who had defended their right to smoke in the presence of nonsmokers showed noticeably less interest in taking part in a discussion on this topic in a train compartment" (p. 44).

Noelle-Neumann claims that the train test also demonstrates the other side of the spiral of silence—a perceived positive climate of opinion will cause individuals holding the socially accepted point of view to be more likely to state their opinion publicly: "Nonsmokers tend to be less self-assured and consequently less inclined to try to make their point of view stick. When . . . [something in their environment] shows them, however, that they are by no means alone in their views, they become noticeably more inclined to join in the conversation" (p. 45).

Noelle-Neumann's hypothesis is simple but has broad applicability. For instance, the spiral of silence provides an explanation for the phenomenon referred to as floating voting: "Repeated questioning of the same people before and after the 1972 election revealed to us that those who feel they are relatively isolated from others . . . are the ones most likely to participate in a last-minute election swing. Those with weaker self-confidence and less interest in politics are also likely to make a last-minute switch" (p. 6).

The lengthiest section of the text is devoted to a review of the classical literature on public opinion ranging from Locke to Lippmann. The purpose of this review is to establish a theoretical basis for the idea that public opinion rests on man's social nature and the resulting fear of isolation. This review may be valuable to those who adopt the book as an undergraduate or graduate text. I would have preferred that it be balanced by a like review of the relevant literature in attitude theory.

*The Spiral of Silence* deserves a place on every public opinion professional's shelf. The scope of the book makes it entirely appropriate as reading for any advanced course in public opinion or voting behavior. It may also be appropriate as text for introductory public opinion courses.

James W. Hottois

*Eastern Oregon State College*

**Language, Form, and Inquiry: Arthur F. Bentley's Philosophy of Social Science.** By James F. Ward. (Amherst: University of Massachusetts Press, 1984. Pp. vii + 270. $24.00.)

Failed university teacher, newspaper editor, orchardist, and philosopher, Arthur Bentley was also the sideline genius of our methodological contests. James Ward's book, valuable for its detailed exposition and careful cultivation of the many volumes of Bentley's writings, will be a standard work for Bentley scholars. Ward does an excellent job of interpreting Bentley in his philosophical and scientific context.

Bentley pursued postpositivist pragmatism to its furthest reaches. Ward details the journey and gives an account of Bentley's frustration. Chapters 3 and 4 on *The Process of Government* shed light on the dynamism and philosophical subtlety of Bentley's view of groups, the shallowness of many current uses of the concept, and the operational problems Bentley left unresolved. Ward illustrates again the problem that Paul Kress pointed to in *Social Science and the Idea of Process* (University of Illinois Press, 1970), "After all, [Bentley] had not only to persuade social scientists of the futility of their present course; he also had to win them to a vision which he could not yet articulate except by primitive example" (p. 214).

Ward differs with Kress in assessing the major significance of Bentley's work as the development of a version of scientific formalism in the style of Georg Simmel. "Formalism for him is best understood as the methodological elimination of ontological claims." Take out all presuppositions and form remains. "Well-developed sciences are bundles of practices that have at their cores sets of agreements having to do with truth and other matters" (p. 226). Ward identifies Bentley's ventures into such terminology as simbiotaxiplasm (combinations of men and "assimilated things" that form society) as examples of his struggle to characterize a suitable formal core for social science. Less specific attention is paid to Bentley and Dewey's formulation of "transactionalism" in *Knowing and the Known* (Beacon Press, 1949), where aspects of a potentially useful formalism are noted, as in the "conditions for naming" (*Knowing. . . ,* pp. 48-49), the glossary of "suggested experimental namings" (pp. 72-74), and the use of transactionalism to reinterpret natural-ism (p. 104). Ward makes clear the philosophical context of transactionalism. It would be interesting to have an analysis of applications of transactionalism as a further test of the utility of Bentley's formalism.

In Ward's view, Bentley's failure is that his "methodological teaching does not liberate itself" from the subject of study (p. 232). Ward characterizes this as "contemplative resignation." The phrase suggests that a "liberation" is possible—and desirable. Ward adopts the cause of moral philosophy, and of science, as enterprises having intrinsic value.

Bentley's relentless skepticism made him wary, committed though he was to making the world intelligible. As Bentley remarks in a chapter he wrote for *Knowing and the Known,* "Logics of the types we have been examining flutter and evade, but never attack directly the problem of sorting out and organizing words to things, and things to words, for their needs of research. They proceed as though some sort of oracle could be issued to settle all puzzles at once, with logicians as the priests presiding over the mysteries" (pp. 38-39). Ward rightly argues that Bentley never made it to the priesthood either. Someone who holds that "knowings are behaviors" (*Knowing. . . ,* p. 74) is a poor candidate in anybody's religion. Yet, as Ward himself demonstrates, Bentley is at least the most creative agnostic of the twentieth century—and that has intrinsic value in a field where neither science nor moral philosophy have yet secured an indisputable claim.

The final chapter, a tightly written review of the argument and critique of Bentley, provides an excellent guide to this intricate and stimulating exploration of Bentley. Ward's scholarship is exemplary.

KENNETH R. HOOVER

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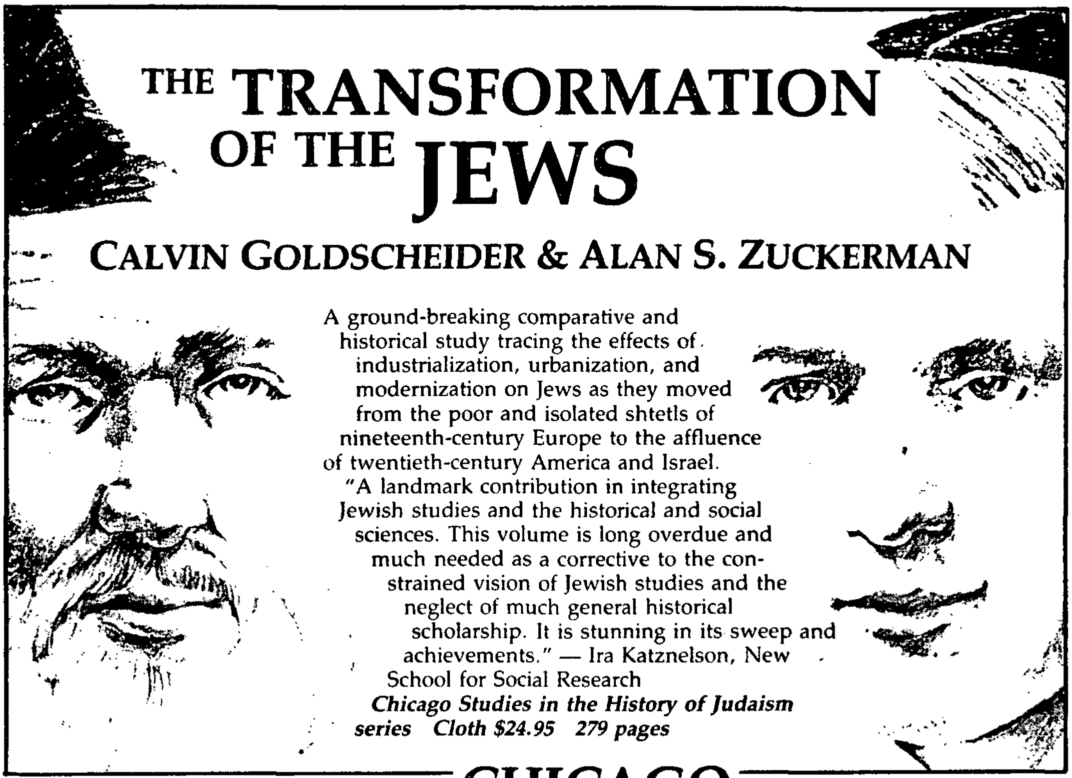

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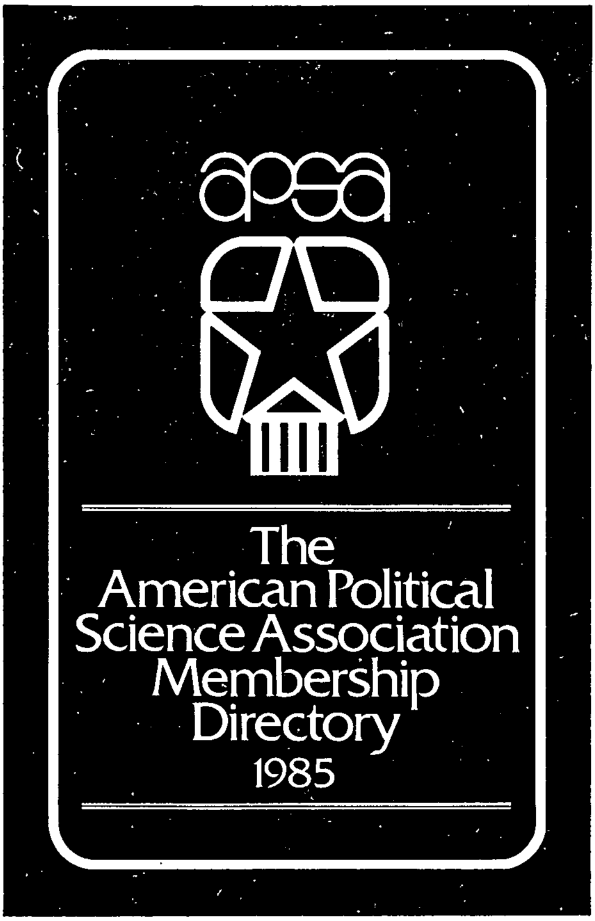

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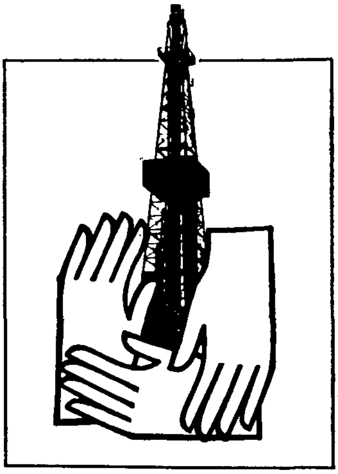

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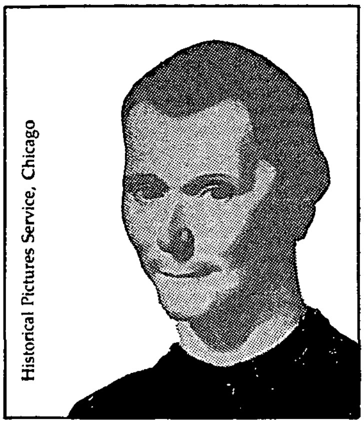

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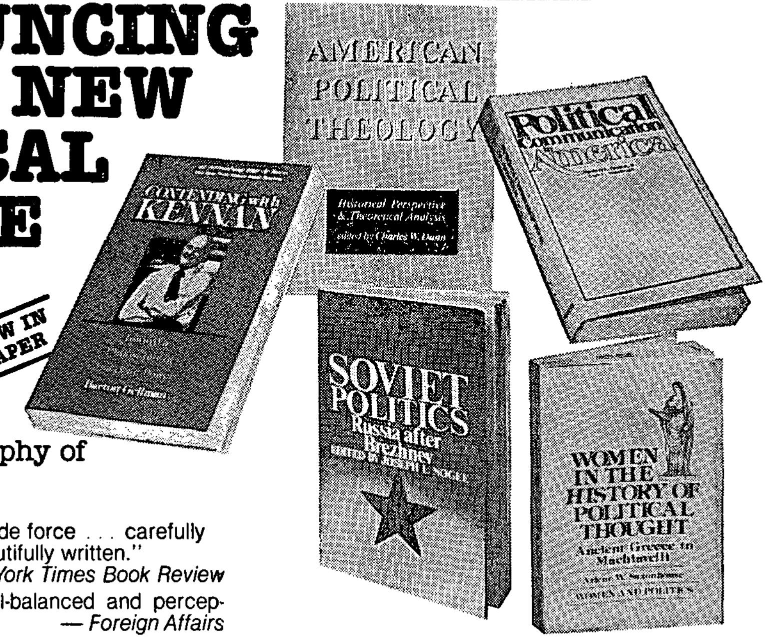
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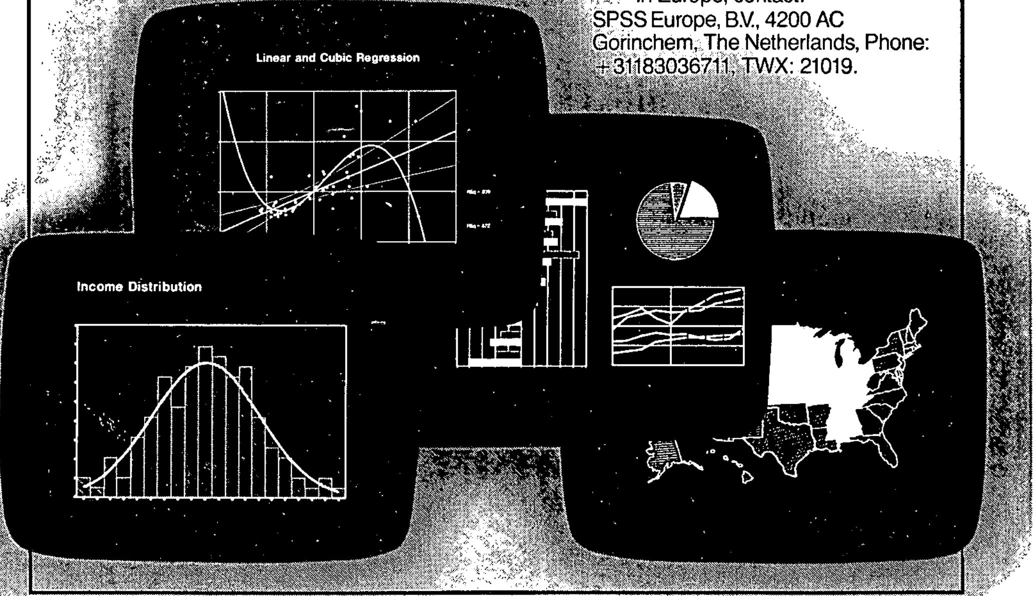

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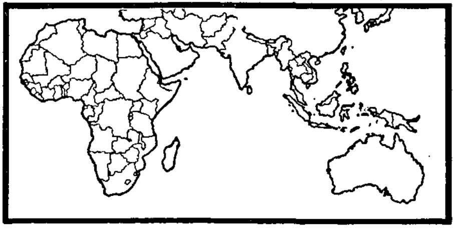

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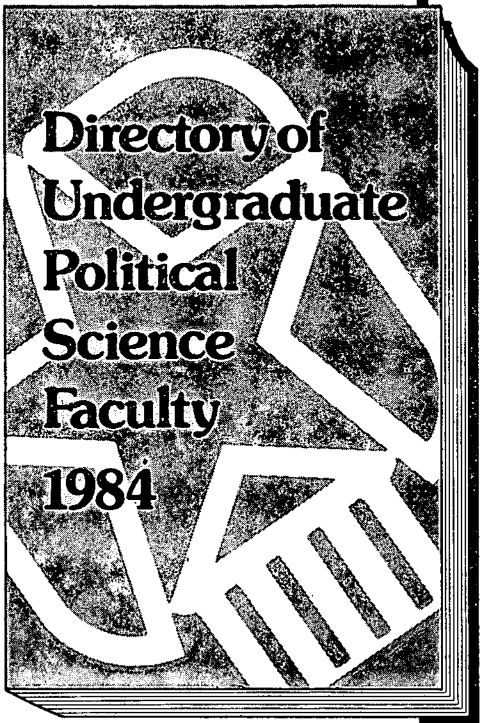

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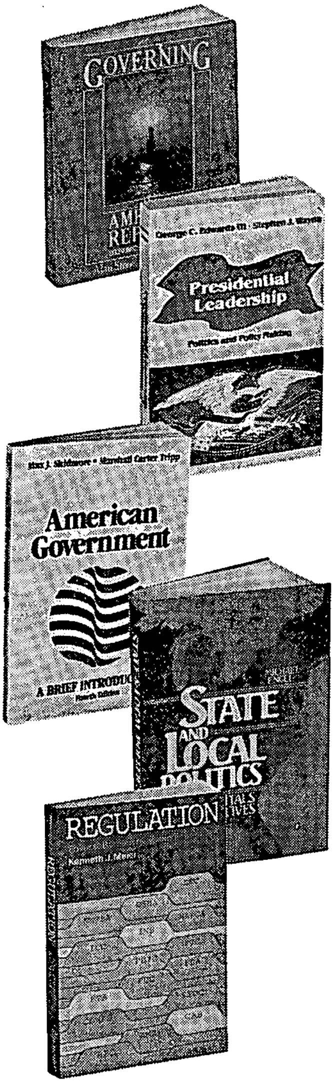

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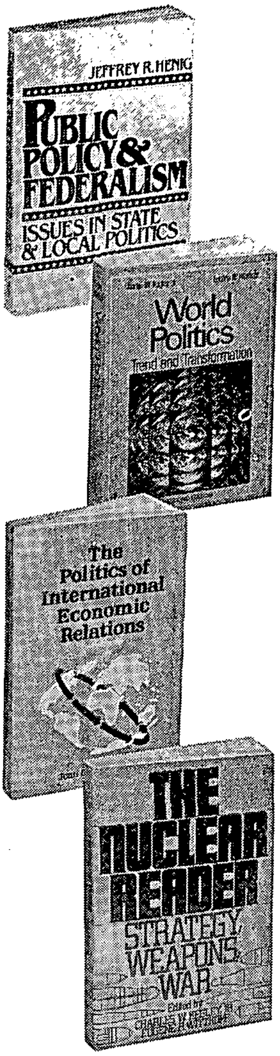
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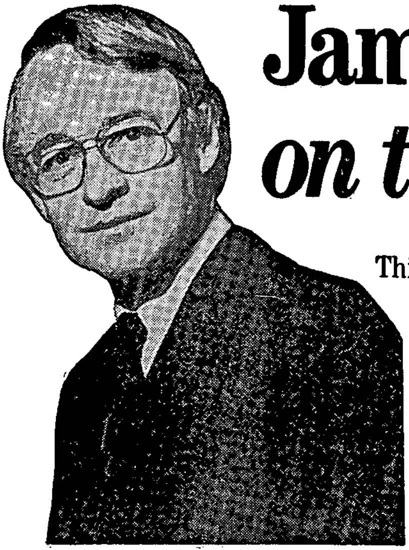

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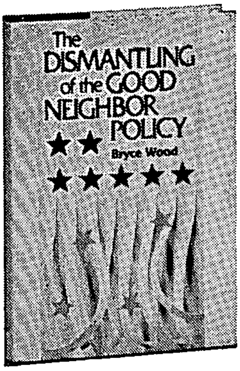

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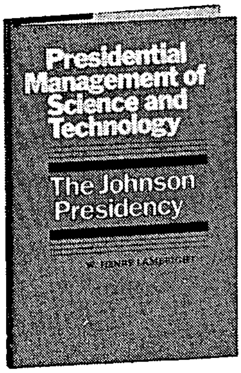

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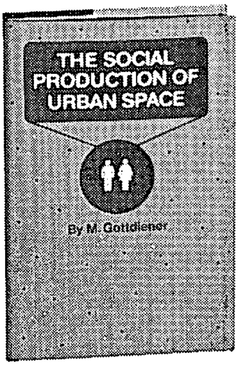

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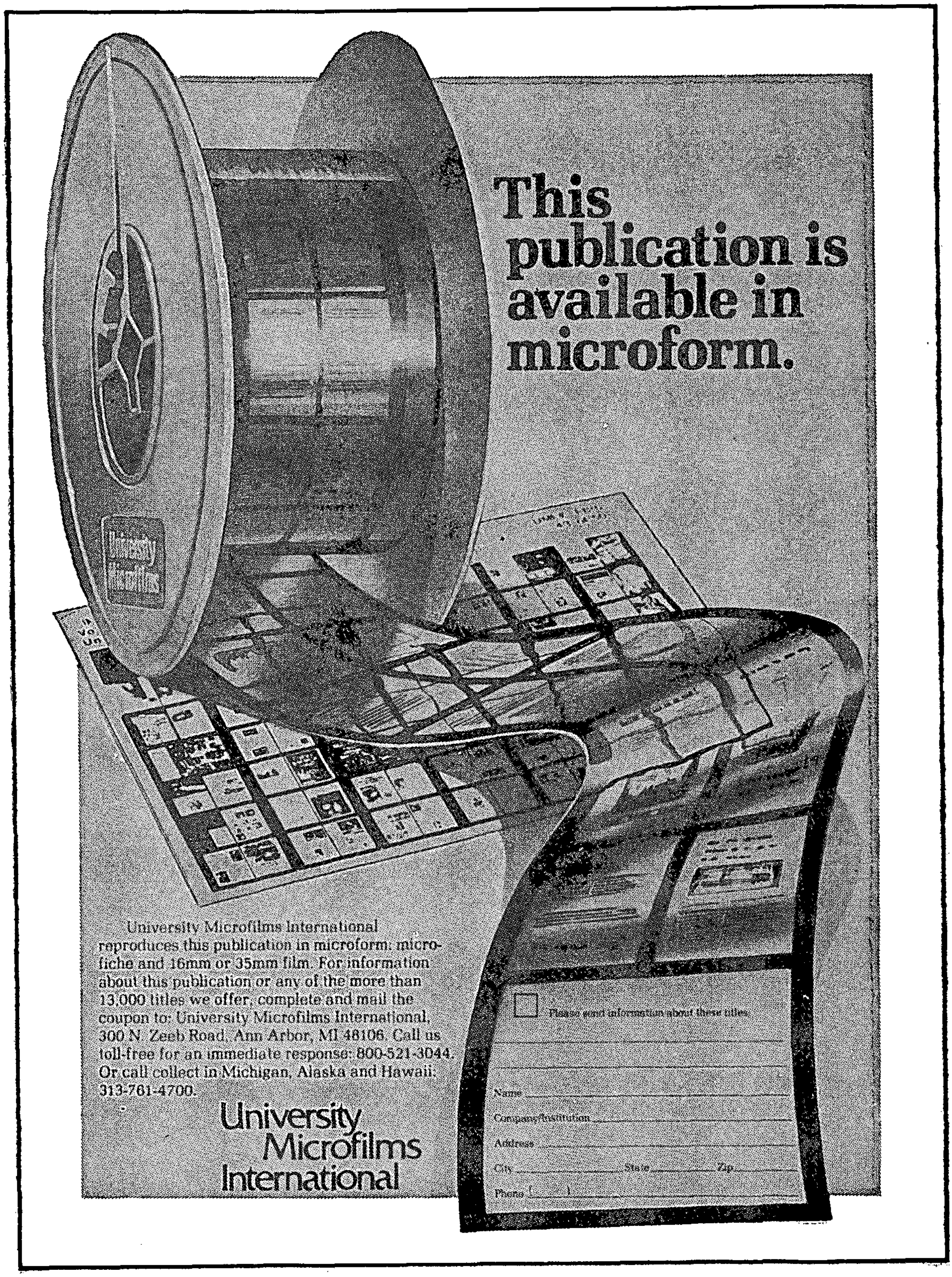

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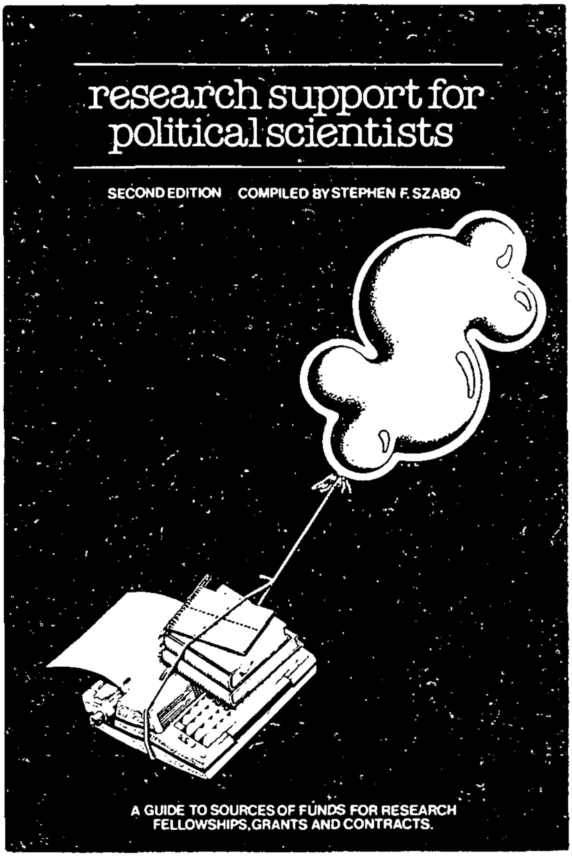

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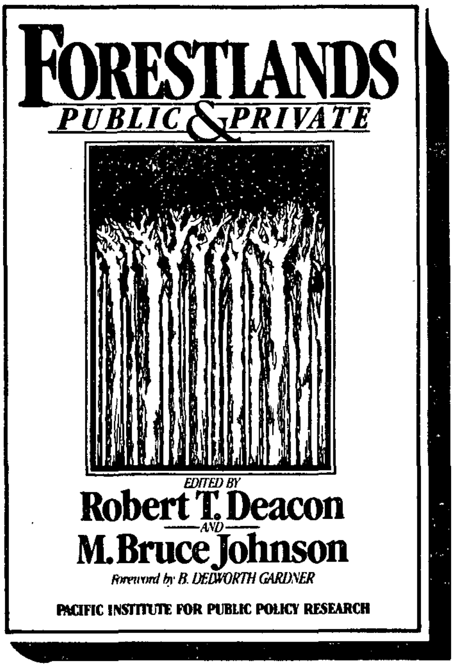
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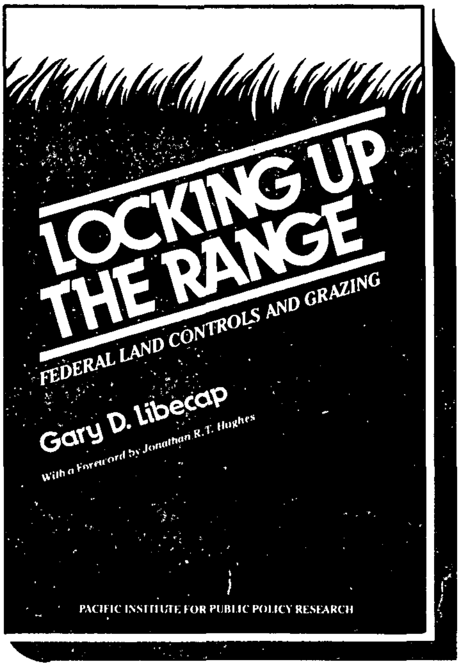
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